PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCLXXVII.

VOL. 167.—PART I.

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PRINTED BY TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET.
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But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the

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thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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PHILOSOPHICAL TRANSACTIONS.

I. *On the Determination of* VERDET'S *Constant in Absolute Units.* By J. E. H. GORDON, *B.A., Gonville and Caius College, Cambridge.*—*1st and 2nd Memoirs**. *Communicated by* J. CLERK MAXWELL.

Received June 5,—Read June 15, 1876.

[PLATE 1.]

[NOTE.—The whole of this work has been done under Professor CLERK MAXWELL'S superintendence; he suggested the method and nearly all the details, and any merit which the investigation may have belongs to him. He is, however, in no way responsible for any errors there may be in the numerical results.]

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* In a former memoir which Prof. MAXWELL did me the honour of communicating to the Royal Society, and which was read June 17th, 1875, the object of this research was explained and a result given.

The method employed, however, was faulty in several respects, the two most important of which were:—

1st. The rotations produced were exceedingly small, so that a slight error in the determination of the plane of polarization introduced a large error into the results.

2nd. A tangent-galvanometer was used to determine the strength of the current.

In the experiments which are the subject of the present Memoir I have, at Prof. MAXWELL'S suggestion, used

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INTRODUCTORY.

In the year 1845 **Faraday** discovered that if plane polarized light passes through certain media, and these media be acted on by a sufficiently powerful magnetic force, the plane of polarization is rotated.

About the year 1853 M. **Verdet** commenced a long and exhaustive examination of the subject, and his first result was published in Ann. de Chimie et de Phys. 3 sér. tom. xli.

In **Maxwell**'s 'Electricity,' Art. 808, vol. ii. p. 400, M. **Verdet**'s results are summarized as follows:—

The angle through which the plane of polarization is turned is proportional

(1) To the distance which the ray travels within the medium;

(2) To the intensity of the resolved part of the magnetic force in the direction of the ray.

(3) The amount of rotation depends on the nature of the medium.

Prof. **Clerk Maxwell** then goes on to say

"These three statements are included in the more general one that the angular rotation is numerically equal to the amount by which the magnetic potential increases from the point at which the ray enters the medium to that at which it leaves it *multiplied by a coefficient* which for diamagnetic media is generally positive."

The object of the present research is to determine the coefficient for a particular medium and for light of a particular wave-length.

"**Verdet**'s constant" is this coefficient for a particular medium and ray, or, in other words, it is the angular rotation produced on that ray and in that medium by a difference of magnetic potential equal to unity.

In order that the measurements may be expressed in absolute units, it is necessary to modify M. **Verdet**'s mode of proceeding in several respects.

In particular an electromagnet with an iron core is unsuitable for this investigation, for both the amount and the distribution of the magnetic force between the poles depend on the properties of the iron core, and cannot be deduced from the strength of the current in the helix. **Faraday**'s heavy glass and other media having the highest power of rotating the plane of polarization were also unsuitable to be used as standard media on account of the difficulty of procuring specimens exactly alike. The following method was therefore adopted:—

The magnetic force was produced by means of an electric current in a helix without an iron core, and bisulphide of carbon enclosed in a tube with glass ends placed within the helix was chosen as the medium. The constants of the helix were measured by the methods described in pages 4–14.

The strength of the current in it was deduced from the deflection of a small magnet

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bisulphide of carbon instead of water, which was used in the earlier experiments, thus increasing the rotation, and have made the helix act as its own galvanometer by suspending a magnetized needle in its neighbourhood.

In consequence of these improvements in the methods, I have, through Prof. **Maxwell**, requested the Council of the Royal Society to permit me to withdraw my first Memoir, and have at the same time embodied in the present paper those portions of the first which are necessary for a due understanding of the second.

suspended near to it and outside it, and the rotation was measured by means of a divided circle.

The methods by which the light was polarized and analyzed are described later. The investigation then resolved itself into three parts:—

(1) The determination of the constants of the helix.

(2) The determination of the ratio which the rotation per unit of length bore to the tangent of the deflection of the suspended needle.

(3) The determination of the horizontal component of the earth's magnetism at the time and place of observation.

The greater number of the experiments were made in my laboratory at Pixholme, near Dorking, but the determination of the number of windings and part of the determination of the area of the helix were made in the Cavendish laboratory in Cambridge.

DESCRIPTION OF INSTRUMENTS EMPLOYED.

The helix was about 26·34 centims. long, about 5 centims. internal diameter, and 12·5 centims. external, and contained about 35 lbs. of No. 20 cotton-covered copper wire. The insulation-resistance was exceedingly high, sufficiently so to enable me to neglect leakage. The resistance of the helix was 1·01 B.A. unit.

The *small dynamometer* consisted of an ebonite ring, about 12 centims. diameter, on which were wound six turns of wire. It was made for me by Messrs. ELLIOTT, and the mean diameter of the coils accurately gauged.

The *large dynamometer* was the instrument belonging to the British Association. It is described and figured in MAXWELL'S 'Electricity,' vol. ii. fig. 54, p. 330. (The suspended coil there shown was not used.)

The polarizing apparatus consisted of a Nicol's prism, rather more than 1 centim. diameter, fixed into a collimating tube taken from a spectroscope. The slit was removed, and the Nicol substituted in its place.

The analyzing apparatus.—This, together with its circle and the Nicol, is the property of the Cavendish laboratory, and was kindly lent to me by Professor MAXWELL. The analyzer was a Jellett prism, described in the next paragraph. It was mounted in a tube, together with a lens and some diaphragms, which gave very good definition (see Plate 1).

The Jellett prism[*] consisted of a piece of Iceland spar, of which the ends were cut normal to the sides. It was then divided by a plane, making an angle of about 1° with the plane containing the two long diagonals. One half was reversed, and the two cemented together again.

The prism was mounted in a brass tube, and a diaphragm, with a hole of some 3 or 4 millims. diameter in it, was placed across one end. On looking in from the other end of the prism, the hole was seen by the ordinary rays in the form of a circle divided by a line across it, the light of the two semicircles being polarized in planes making with each other an angle of about 2°.

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[*] This prism is described by Prof. JELLETT in the 'Transactions of the Royal Irish Academy,' vol. xxv. Prof. JELLETT also had the kindness to advise the author as to the form of his instrument best suited to this investigation.

The image of the hole formed by the extraordinary rays consisted of two semicircles, one to the right and the other to the left of this circle.

A second diaphragm hid the latter. Now when light that has passed through a Nicol is examined with the prism, and the latter turned so that the light of one half of the circle is extinguished, the other half is slightly illuminated. If the Jellett be turned through rather less than two degrees, the second half of the circle will become dark, and the first will be slightly illuminated.

It is obvious that there is a position between these two where the illumination over the whole circle is uniform, and this position can be observed with considerable accuracy.

With the arrangements that were used in this work the probable error was about 1′.

The divided circle was made for this investigation by Mr. BROWNING, and is about 14 centims. diameter. The circle turns against two fixed verniers. It is divided on brass to 1°, and the verniers read to $\frac{1}{20}$°, *i. e.* to 3′. By estimation 1′ can be read with perfect accuracy. It is moved by a screw of long pitch, working in a thread cut round its edge. This gives a sufficiently quick motion to enable changes in the illumination of any part of the field to be noted, and is yet capable of very delicate adjustment.

The galvanometer consisted of a minute suspended magnet and mirror, weighing about 1 grain, acted on by the helix (outside of which it was placed), and observed by a telescope and scale.

The tube for containing the bisulphide was of brass, with glass ends, and projected some 20 centims. from each end of the helix.

The experimenting table was built of massive brickwork, with a carefully levelled top of fine cement. It was about 1·4 metre square. A continuation of it supported the suspended magnet, and a separate brick pier the telescope by which the magnet was observed (see Plate 1).

The apparatus for measuring horizontal distances consisted of a number of conical plumb-bobs, which hung by silk threads from laths laid across a horizontal deal frame the same size as the table, and fixed about 1 metre above it. Distances projected on the table by these were taken by compasses, and measured on a millim. scale engraved on a massive brass plate by ELLIOTT (for another purpose).

The base of the circle, the telescopes, &c. were all placed on pillars of brickwork built on the tables, and bedded either in cement or plaster of Paris. The result was a steadiness such as I have never before obtained.

THE EXPERIMENTS.

Determination of the Number of Windings.

We must first determine the difference of magnetic potential at the two ends of the helix when a unit current passes through it. It is a quantity which we call N, and is a function of the number of windings and their arrangement; for if we know the magnetic intensity at each point of the axis of the helix due to a unit current, viz. what force is exerted by a unit current in the helix on a unit magnetic pole at that point, then we know how much work would have to be done to move this unit pole from one end of the helix to the other when a unit current was passing in it.

But if a, b be the ends and u_x the force at any point, then the above amount of work would be equal to

$$\int_a^b u_x dx = V_b - V_a, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where V is the potential at any point.

But the dimensions of magnetic potential are, in the electromagnetic system,

$$[L^{\frac{1}{2}}M^{\frac{1}{2}}T^{-1}];$$

and these are also the dimensions of the strength of an electric current.

∴ N, which is the ratio of the former of these things to the latter, *is a number.*

The value of N for the helix was determined by comparison with the great dynamometer of the British Association, which is deposited in the Cavendish Laboratory.

The intensities of the magnetic action were compared at 7 equidistant points in the axis of the helix, and the total force was obtained by integrating by WEDDLE's formula (BOOLE's 'Finite Differences,' p. 47), viz.

$$\int_0^{6h} u_x\, dx = \tfrac{3}{10}h\{u_0 + u_2 + u_4 + u_6 + 5(u_1 + u_5) + 6u_3\}^{*}, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where h is the distance between any two of the points, and u_x is the magnetic intensity at any point in terms of that of the dynamometer.

The mechanical arrangements were as follows:—the dynamometer, which consists of two coils about 50 centimetres in diameter, at a distance of about 25 centimetres one from the other, was placed so that the plane of the coils was accurately vertical and in the magnetic meridian.

The axial line of the coils was then carefully found and marked by means of plumb-lines and cross-threads fixed to the table.

A strong T-shaped board supported on three levelling-screws was placed so that the part corresponding to the stem of the T passed through the coils in a horizontal plane parallel to their axis; on this the helix was laid and its axis brought into exact coincidence with that of the coils.

A boxwood cylinder about 20 centims. long was turned to fit nicely into the helix; a long thick brass wire terminating in a handle was fixed into one end, and a brass pin about 5 centims. long was fixed near one edge of the other.

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[*] Objections have been taken to the use of this formula, which gives, it is observed, much more weight to u_1 and u_5 than to u_2 and u_4, and does not furnish an approximation of a legitimate analytical character. While fully acknowledging the force of these objections, I have not thought it worth while to make an alteration which would involve repeating most of the arithmetic in the paper, for this reason: the experiments, being made with resistance-coils, are susceptible of such close accuracy that the errors of any particular determination, even when multiplied by 5, cannot perceptibly affect the value of N deduced.

To the end of the latter a magnet and mirror weighing only about a grain was hung by a silk fibre, so that when the pin was at the highest point the magnet hung in the axis of the cylinder. The helix being placed coaxial with the coils was pushed endways so that the centre point of the coils was just outside one end of it.

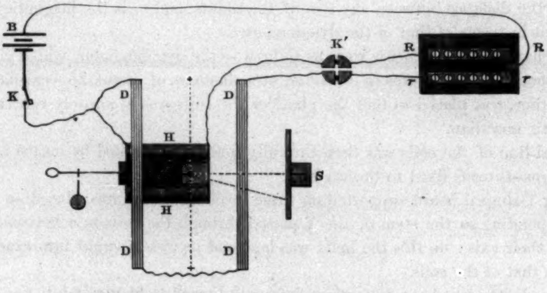
The cylinder was then inserted and adjusted, by means of cross-wires, so that the mirror hung exactly at the centre point, and a mark was then made on the handle corresponding to a mark on the stand of the instrument, by reference to which the magnet could always be brought to the same position.

Thus while the helix was slid along the axis, the magnet inside it could always be placed at the centre point of the dynamometer. The distance between the inside ends of the helix was divided into six equal parts by means of seven pins stuck as sights into a slip of wood fixed along the top edge. Thus by sliding the helix along till any one of the sights was between the vertical threads placed midway between the coils, the force at that point due to the helix could be compared with that due to the dynamometer by means of the needle at the centre.

The comparison was made by sending currents of different intensities through the helix and coils in opposite directions, and varying them till there was no action on the needle.

This was effected by dividing a current and passing one portion through the coils and the other through the helix, and interposing different resistances in each branch.

The annexed diagram shows the connections.



B is the battery, R and r the resistance-coils.
D, the dynamometer-coils. } And let these letters also represent
H, the helix. } their resistances.
● the magnet and mirror, whose angular position is observed by
S, the scale and lamp.
K K′ are contact-keys.

Now when the actions on the needle are equal, the powers of the coils are inversely as the currents in them, that is, directly as the resistances $r+\mathrm{D}$ and $\mathrm{R}+\mathrm{H}$.

The observations being repeated at each of the seven points, we have the intensity of the magnetic action of the helix at each of those points in terms of that of the dynamometer.

Thus if P be the power of the dynamometer with unit current, and u_x the power of the helix with the same current at a point x, then the total force of the helix (that is, the difference of magnetic potentials at its ends) is

$$N \equiv \int_0^{6h} u_x\,dx = \frac{3h}{10}P\left\{\frac{H+R_0}{D+r_0}+\frac{H+R_{2h}}{D+r_{2h}}+\frac{H+R_{4h}}{D+r_{4h}}+\frac{H+R_{6h}}{D+r_{6h}}\right.$$
$$\left.+5\left(\frac{H+R_h}{D+r_h}+\frac{H+R_{5h}}{D+r_{5h}}\right)+6\,\frac{H+R_{3h}}{D+r_{3h}}\right\}, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where h is $\frac{1}{6}$ the length of the helix.

In the helix used the following results were obtained:—

$$h=4{\cdot}39 \text{ centims.}, \quad u_x=P\frac{R+H}{r+D}, \quad \begin{array}{ll} H=\ 1{\cdot}01, & \text{take}=1{\cdot}00, \\ D=28{\cdot}15 & \quad ,, \quad\ 28{\cdot}1. \end{array}$$

	$r=100.$	$r=200.$	$r=1000.$
u_0	$P\frac{348+H}{100+D}=2{\cdot}724\ P$	$P\frac{629+H}{200+D}=2{\cdot}762\ P$	$P\frac{2797+H}{1000+D}=2{\cdot}722\ P$
u_1	$P\frac{636+H}{100+D}=4{\cdot}973\ P$	$P\frac{1134+H}{200+D}=4{\cdot}976\ P$	$P\frac{5100+H}{1000+D}=4{\cdot}962\ P$
u_2	$P\frac{725+H}{100+D}=5{\cdot}667\ P$	$P\frac{1290+H}{200+D}=5{\cdot}659\ P$	$P\frac{5840+H}{1000+D}=5{\cdot}681\ P$
u_3	$P\frac{739+H}{100+D}=5{\cdot}777\ P$	$P\frac{1315+H}{200+D}=5{\cdot}725\ P$	$P\frac{5960+H}{1000+D}=5{\cdot}798\ P$
u_4	$P\frac{713+H}{100+D}=5{\cdot}574\ P$	$P\frac{1270+H}{200+D}=5{\cdot}572\ P$	$P\frac{5750+H}{1000+D}=5{\cdot}592\ P$
u_5	$P\frac{615+H}{100+D}=4{\cdot}808\ P$	$P\frac{1097+H}{200+D}=4{\cdot}814\ P$	$P\frac{4969+H}{1000+D}=4{\cdot}639\ P$
u_6	$P\frac{348+H}{100+D}=2{\cdot}724\ P$	$P\frac{620+H}{200+D}=2{\cdot}722\ P$	$P\frac{2799+H}{1000+D}=2{\cdot}723\ P$

Hence we have for the values of u_x, by taking the means of the above,—

r.	u_0.	u_1.	u_2.	u_3.	u_4.	u_5.	u_6.
$r=100$	2·724 P	4·973 P	5·667 P	5·777 P	5·574 P	4·808 P	2·724 P
$r=200$	2·762 P	4·976 P	5·659 P	5·725 P	5·572 P	4·814 P	2·722 P
$r=1000$	2·722 P	4·962 P	5·681 P	5·798 P	5·592 P	4·639 P	2·723 P
Mean ...	2·736 P	4·970 P	5·669 P	5·766 P	5·579 P	4·754 P	2·723 P

Hence

$$N=\int_0^{6h} u_x\,dx=\tfrac{3}{10}\,4{\cdot}39 \text{ centims.}\{16{\cdot}707+48{\cdot}620+34{\cdot}596\}\mathrm{P}$$

$$=131{\cdot}732[\mathrm{LP}]\mathrm{P_D}^*.$$

Now $$\mathrm{P_D}=81{\cdot}1620,$$

$$\therefore\ 131{\cdot}732\,\mathrm{P_D}=10751{\cdot}96.$$

Now in order that this may be a number it is necessary that [P] should equal $[\mathrm{L}^{-1}]$; and if we consider the equation of moments we shall see that this is so. For consider a small magnet (length $2l$) at the centre of the dynamometer, we have, when a current passes,

$$\mathrm{H}\sin\delta\, l\,[\mathrm{H\,.\,L}]=\mathrm{PC}\cos\delta\, l\,[\mathrm{PC\,.\,L}],$$

i.e.

$$[\mathrm{H}]=[\mathrm{P\,.\,C}],$$

or

$$[\mathrm{P}]=\left[\frac{\mathrm{H}}{\mathrm{C}}\right]=\frac{[\mathrm{L}^{-\frac{1}{2}}\mathrm{M}^{\frac{1}{2}}\mathrm{T}^{-1}]}{[\mathrm{L}^{\frac{1}{2}}\mathrm{M}^{\frac{1}{2}}\mathrm{T}^{-1}]}=[\mathrm{L}^{-1}]. \qquad (4)$$

Now the value of N for any helix with unit current taken with respect to the whole length of its axis produced to an infinite length in both directions is, by Art. 676 of Professor CLERK MAXWELL's 'Electricity,' $4\pi n$, where n is the number of windings.

When the length l is finite compared with the radius a, the value of N for that part of the axis which is included between the ends is

$$\mathrm{N}=4\pi n\,\frac{\sqrt{l^2+a^2}-a}{l} \qquad (5)$$

(see CLERK MAXWELL's 'Electricity,' Art. 676).

Now if we calculate $n=\frac{\mathrm{N}}{4\pi}\,\frac{l}{\sqrt{l^2+a^2}-a}$, taking a the mean radius, this will give the number of windings in the helix.

Now as $a=4{\cdot}84$ centims., and $\mathrm{N}=10752$, we have

$$n=\frac{10752}{4\pi}\cdot\frac{26{\cdot}34}{\{26{\cdot}34^2+4{\cdot}84^2\}^{\frac{1}{2}}-4{\cdot}84};$$

$$\therefore\ \log n=\log 10752+\log 26{\cdot}34-\log 4\pi-\log\overline{(l^2+a^2)^{\frac{1}{2}}-a}$$

$$\{\text{last term}=\log 21{\cdot}92\}$$

$$=4{\cdot}0314893+1{\cdot}4206158-1{\cdot}0991971-1{\cdot}3408405$$

$$=3{\cdot}0120680,$$

$$\therefore\ n=1028{\cdot}15,$$

$\therefore$ there are 1028 windings on the helix.

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[*] Professor CLERK MAXWELL's method of representing the units by quantities in [ ] has been used throughout the paper. Thus a length L is represented by L[L], where L is a number and [L] the unit of length.

Verification.

Now in the length of the helix there are 91 windings on the outside layer, and the ratio of the length to the difference of the internal and external radii is $\frac{26\cdot34}{3\cdot47}=7\cdot5$ about.

∴ assuming that the number of layers per centim. of radius is the same as the number of windings per centim. of length (it would really be a little greater, as they fit into each other, $^{\circ\circ\circ\circ}_{\circ\circ\circ\circ}$), we have

$$n=\left\{91\times\frac{91}{7\cdot5}\right\}=1092,$$

which is sufficiently near the calculated result, viz. $n=1028$, to show that no large mistake has been made, as, for instance, writing down a log with a wrong index.

Closer agreement could hardly be expected, as the helix was made for a different purpose, and no particular pains were taken to wind it uniformly. It is also probable that the instrument-maker took more pains to wind the wires of the outside layer, which could be seen, close together than those of the inside ones, which were hidden.

Determination of Areas.

1*st method.*—To calculate the strength of a current in a helix from the deflection of a needle placed outside it, it is necessary to know $\Sigma(A)$ where A_k is the area of any winding.

To find $\Sigma(A)$ the helix was so placed that a vertical plane through its centre, and normal to its axis, contained the magnet and mirror when only acted on by the earth's magnetism. In this plane, between the helix and magnet, and so arranged that it could be slid along in it, was placed the small dynamometer. The centres of magnet, dynamometer, and helix were in the same horizontal line.

The same current was sent in opposite directions through both helix and small dynamometer[*], and the latter moved till there was no action in the magnet. The distances of the centres from the magnet were then found[†] to be :—

Centre of helix to magnet	82·80 centims.
„ dynamometer to magnet . . .	18·10 „

2*nd method.*—From these data a value of the area was obtained. On looking through these experiments, however, Professor MAXWELL was of opinion that the dynamometer used was not large enough to ensure accuracy, so he had the kindness to compare the helix with the great B.A. dynamometer at Cambridge. In his experiments the helix and dynamometer were placed concentric, and different currents were sent opposite ways through both, till there was no action on a small magnet suspended at a point O in a vertical plane perpendicular to and bisecting the common axis of the dynamometer and helix. Professor MAXWELL found that when from O to centre was so great that the diameters of the coils could be neglected in comparison, the currents producing equilibrium had the ratio

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[*] The circular base of the dynamometer was cemented to a square piece of glass, which slid on a little wooden stand, furnished with a guide-bar lying in the magnetic meridian.

[†] See paragraphs on measurement of horizontal distances and determination of magnetic meridian, pp. 4 & 16.

$$1:11{\cdot}23.$$

From these data another value of the area was calculated. In both cases the following reasoning was used:—

Calculation of $\Sigma(\mathrm{A})$, *the sum of the areas of the windings of the Helix.*

We proceed as follows:—

(1) We first obtain an expression for the force exerted at a point by one winding, at a certain distance and of a certain area, carrying a unit current.

(2) We then, by calculation, find what the action of a certain standard coil of known area would be with a unit current.

(3) We then, by experiment, find what that action is with a certain arbitrary current whose ratio to a second current is known.

(4) We then, by experiment, find what the action of the helix is with this second current, adjusting the ratios till the actions are equal.

(5) This eliminates the variations of the current, and enables us, by means of expression (1), to compare the areas acting in (3) and (4); one of these being known, we can obtain the other.

To find an expression for the force exercised by a circular current in a given direction at a point, we must first find the potential at that point, and then differentiate along the given direction.

For a winding carrying a current of strength i we may substitute a magnetic shell bounded by the winding and of strength i. But by MAXWELL'S 'Electricity' (Art. 409), the potential at any point due to a magnetic shell is

$$V=\Phi\omega, \qquad (6)$$

where Φ is the strength of the shell and ω the solid angle subtended by it at the point.

We have therefore to find an expression for ω.

Now in Arts. 670 and 694 we find

$$\omega=\frac{1}{c}\frac{d}{dr}\cdot(\mathrm{P}r); \qquad (7)$$

for when Φ=unity, $V=\omega$, where radius of winding (which in this experimental case becomes a great circle)$=c$. r is the distance from the centre of the circle to the point where the potential is to be found, and P the potential of a stratum of matter of surface density unity spread over a hemisphere bounded by the circle. Expanding this, we obtain in Art. 695 the expression for the potential of a point outside the circle,

$$\omega' = 2\pi \sin^2\alpha \left\{ \frac{1}{2} \frac{c^2}{r^2} Q'_1(\alpha) Q_1(\theta) + \&c. + \frac{1}{i+1} \cdot \frac{c^{i+1}}{r^{i+1}} \cdot Q'_i(\alpha) Q_i(\theta) \right\}, \quad . \quad . \quad (8)$$

where α and θ are the angles in Art. 694.

Let xy be the plane of the coil; then the force perpendicular to it is obtained by differentiating the potential with regard to z, and

$$z = r\cos\theta = r\mu.$$

Then for the force we get, changing the independent variables from r, μ, to x, y, z,

$$\frac{d\omega'}{dz} = \frac{d\omega'}{dr} \cdot \frac{dr}{dz} + \frac{d\omega'}{d\mu} \cdot \frac{d\mu}{dz}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and remembering that

$$r = \sqrt{x^2 + y^2 + z^2}, \quad \frac{dr}{dz} = \frac{z}{r} = \mu,$$

$$\mu = \frac{z}{\sqrt{x^2 + y^2 + z^2}}, \text{ and } \frac{d\mu}{dz} = \frac{r - \frac{z^2}{r}}{r^2} = \frac{1}{r}(1 - \mu^2),$$

$$\frac{d\omega'}{dz} = \mu \frac{d\omega'}{dr} + (1 - \mu^2) \frac{1}{r} \cdot \frac{d\omega'}{d\mu}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

But when, as in this case, the point where the potential is required is in the plane of the coil, we have α and θ each $= \frac{\pi}{2}$, which gives $\mu = 0$, and reduces the expression (10) to

$$\text{Force perpendicular to plane of coil} = \frac{d\omega'}{dz} = \frac{1}{r} \cdot \frac{d\omega}{d\mu}. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

Now by Art. 694, p. 301,

$$\frac{d}{d\mu} \cdot Q_i(\theta) = Q'_i(\theta). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

The values of this are given in Art. 698, and in them we must put $\mu = 0$. Hence the force for one winding is

$$\frac{d\omega'}{dz} = \frac{2\pi}{r} \left\{ \frac{1}{2} \frac{c^2}{r^2} \left[Q'_1\left(\frac{\pi}{2}\right) \right]^2 + \frac{1}{3} \cdot \frac{c^3}{r^3} \left[Q'_2\left(\frac{\pi}{2}\right) \right]^2 + \&c. \right\}$$

$$= \frac{2\pi}{r} \left\{ \frac{1}{2} \cdot \frac{c^2}{r^2} \cdot (1)^2 + \frac{1}{4} \frac{c^4}{r^4} \cdot \left(\frac{3}{2}\right)^2 + \frac{1}{6} \frac{c^6}{r^6} \cdot \left(\frac{15}{8}\right)^2 + \&c. \right\}$$

$$= \frac{\pi}{r} \left(\frac{c^2}{r^2} + \frac{1}{2} \frac{c^4}{r^4} \left(\frac{3}{2}\right)^2 + \frac{1}{3} \cdot \frac{c^6}{r^6} \left(\frac{15}{8}\right)^2 \right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

This is the force exercised by one winding of radius c on a unit magnetic pole in its plane distant r from its centre, when the winding carries a unit current.

The area of the winding is

$$A=\pi c^2.$$

The force exercised when the strength of the current is C is of course $C\frac{d\omega'}{dz}$. Then we may write

$$F_0=\frac{\Sigma(A)}{r^3}\left(1+\tfrac{9}{16}\frac{c^2}{r^2}+\tfrac{75}{128}\cdot\frac{c^4}{r^4}+\ldots\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

where F_0 is the force on a unit pole due to a unit current.

But the small dynamometer consists of windings all of the same diameter, and approximately all in one plane, and therefore this expression is so nearly true that it may be used for it. When thus used it will be distinguished by dashes on the letters.

The second and third terms are required because in the experiments the small dynamometer is placed very close to the suspended magnet.

The effect of the helix may be got by a much simpler method (see MAXWELL, Art. 676). Let $2l$ be the length of the helix, c its mean radius, then the outside effect at the distance of (say) a metre or more is that of a disc of (+) magnetism at one end and of (−) magnetism at the other.

For unit current the moment of the magnet is $\Sigma(A)$; hence its strength is $\frac{\Sigma(A)}{2l}$, which is the quantity of magnetism at each end.

Then the value of the potential will be $\frac{\Sigma A}{2l}\left(\frac{1}{r_1}-\frac{1}{r_2}\right)$, and the value of the force at O will be $F_0=\frac{\Sigma(A)}{2l}\cdot\frac{2}{r^2}\cdot\frac{l}{r}=\frac{\Sigma A}{r^3}$ per unit current.

Thus the force $-F'$ exerted by the dynamometer is

$$-F'=-\frac{C'm\Sigma(A)'}{r_{\prime}^3}\left(1+\tfrac{9}{16}\cdot\frac{c_{\prime}^2}{r_{\prime}^2}+\tfrac{75}{128}\frac{c_{\prime}^4}{r_{\prime}^4}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

while F, that exerted by the helix, is

$$F=\frac{C\Sigma(A)m}{r^3} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

But in my experiments $C=-C'$; and when we have no action on the magnet at O, we have

$$F=-F'.$$

Adding the two expressions we have

$$0=\frac{\Sigma(A)}{r^3}-\frac{\Sigma(A)'}{r_{\prime}^3}\left(1+\tfrac{9}{16}\cdot\frac{c_{\prime}^2}{r_{\prime}^2}+\tfrac{75}{128}\frac{c_{\prime}^4}{r_{\prime}^4}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

Now let r, r' be the values of r for the helix and small dynamometer respectively, we have, when the force on the suspended magnet is zero,

$$r^3=(\overline{82\cdot8}^2+b^2)^{\frac{3}{2}}, \text{ where } 2b=26\cdot34 \text{ centims.},$$

and

$$\therefore r^3=\log^{-1} 5\cdot7703411.$$

Also noting that $2b_{\prime}=1\cdot04$,

$$r_{\prime}^{3}=(\overline{18\cdot10}^{2}+\overline{0\cdot52}^{2})^{\frac{3}{2}}=\log^{-1} 3\cdot7735723.$$

Now $\Sigma(A)'=6\pi c_{\prime}^{2}$, where $c_{\prime}$ is the mean radins of the coils.

Now the diameters of the coil are

Internal	. . . .	4·815 inches*,
External	. . . .	4·950 „

which give a mean radius

$$c_{\prime}=6\cdot201 \text{ centims.};$$

whence $\Sigma(A)'=\log^{-1} 2\cdot8602118$.

From (17) we have, then,

$$\Sigma(A)=\frac{\Sigma(A)'r^{3}\left(1+\frac{9}{16}\frac{c_{\prime}^{2}}{r_{\prime}^{2}}+\frac{75}{128}\frac{c_{\prime}^{4}}{r_{\prime}^{4}}\right)}{r_{\prime}^{3}} \quad . \ . \ . \ . \ . \ . \ . \quad (18)$$

Calculating this from the data just given, we have as the result of the small dynamometer experiments

$$\text{Log } \Sigma(A)=4\cdot8880699$$

and

$$\Sigma(A)=77280\cdot5.$$

From these same experimental data Prof. **Maxwell** made a calculation, from which he found

$$\Sigma(A)=77554\cdot0$$

Prof. Maxwell's *Experiments with the large Dynamometer.*

In these experiments the helix and dynamometer were placed exactly concentric. A magnet and mirror was suspended rather more than a metre distant from them, first in front and then behind, so as to correct any error in centering, and varying currents were sent opposite ways till there was no deflection.

At this distance the helix and dynamometer could each be considered to be replaced by their equivalent magnetic discs, and the difference between r and r_1 may be neglected in comparison with those quantities themselves.

The formula for $\Sigma(A)$ then becomes

$$\Sigma(A)=\frac{C'}{C}\,\Sigma(A)'.$$

Where there was no action on the suspended magnet, Prof. **Maxwell** found

$$\frac{C}{C'}=11\cdot23$$

Now the area $\Sigma(A)'$ of the great dynamometer is

$$870200 \text{ sq. centims.}$$

---

* These measurements were made by Messrs. **Elliott**.

This gives

$$\Sigma(A)=77488{\cdot}8 \text{ sq. centims.}$$

Thus we obtain the following values for the area of the helix:—

By Prof. MAXWELL's experiments with the great B.A. dynamometer—

$$\Sigma(A)=77488{\cdot}8 \text{ sq. centims.}$$

By GORDON's experiments with small dynamometer (GORDON's calculation)—

$$\Sigma(A)=77280{\cdot}5 \text{ sq. centims.}$$

By MAXWELL's calculation of GORDON's experiments—

$$\Sigma(A)=77554 \text{ sq. centims.}$$

Mean of the two calculations of GORDON's experiments—

$$\Sigma(A)=77417{\cdot}2 \text{ sq. centims.}$$

Finally,

$$\Sigma(A)=\left\{\begin{array}{ll}\text{MAXWELL} \;.\;.\;.\;. & 77488{\cdot}8 \\ \text{GORDON} \;.\;.\;.\;.\;. & 77417{\cdot}2\end{array}\right\} \text{ Mean } 77453{\cdot}0 \text{ sq. centims.}$$

Calculation of the strength C *of a current in the Helix in terms of the deflection* δ *of the suspended magnet.*

Let $2l$ be the length of the suspended magnet, H the horizontal component of the earth's magnetism, we have from (16)

$$Hml \tan \delta=\Sigma(A)\, \frac{ml}{r^3}\, C$$

or

$$H \tan \delta=\frac{C\Sigma(A)}{r^3},$$

which gives

$$C=\frac{Hr^3}{\Sigma(A)} \tan \delta \quad . \;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;. \quad (19)$$

Formula for ω.

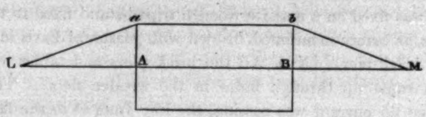
ω is the rotation expressed in circular measure corresponding to a difference of magnetic potential equal to unity. Thus

$$\omega=\frac{\theta}{V_L-V_M}, \quad .\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;. \quad (20)$$

where θ is R expressed in circular measure, and V the magnetic potential at any part of the tube.

The following formula gives the difference of the magnetic potentials at the ends L, M, due to a current C.

If LM were infinite, it would be $4\pi n\mathrm{C}$, where n is the number of windings. When the length of the tube is finite, and equal to LM,



we must subtract the following correction[*]:—

$$\frac{\mathrm{C}\Sigma(\mathrm{A})}{\frac{1}{2}\mathrm{AB}\,.\,\mathrm{A}a^2}\{\mathrm{L}a-\mathrm{LA}-(\mathrm{L}b-\mathrm{LB})+\mathrm{M}b-\mathrm{MB}-(\mathrm{M}a-\mathrm{MA})\}. \quad . \quad . \quad . \qquad (21)$$

When LA is great compared with Aa,

$$\mathrm{L}a-\mathrm{LA}=\frac{1}{2}\frac{\mathrm{A}a^2}{\mathrm{LA}}-\frac{1}{8}\frac{\mathrm{A}a^4}{\mathrm{LA}^3}+\&\mathrm{c.}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (22)$$

or, retaining only the first term, we get

$$\frac{\mathrm{C}\Sigma(\mathrm{A})}{\mathrm{AB}}\cdot\left(\frac{1}{\mathrm{LA}}-\frac{1}{\mathrm{LB}}+\frac{1}{\mathrm{MB}}-\frac{1}{\mathrm{MA}}\right)=\frac{\mathrm{C}\Sigma(\mathrm{A})}{\mathrm{AB}}\left(\frac{\mathrm{AB}}{\mathrm{LA}\,.\,\mathrm{LB}}+\frac{\mathrm{AB}}{\mathrm{MA}\,.\,\mathrm{MB}}\right)$$

$$=\mathrm{C}\Sigma(\mathrm{A})\left(\frac{1}{\mathrm{LA}\,.\,\mathrm{LB}}+\frac{1}{\mathrm{MA}\,.\,\mathrm{MB}}\right). \quad . \quad . \qquad (23)$$

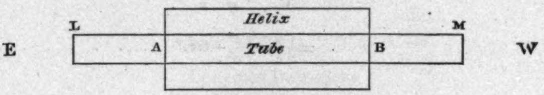
The corrected value is then

$$(\mathrm{V_L}-\mathrm{V_M})=\left\{4\pi n-(\Sigma(\mathrm{A}))\left(\frac{1}{\mathrm{LA}\,.\,\mathrm{LA}}+\frac{1}{\mathrm{MA}\,.\,\mathrm{MB}}\right)\right\}\mathrm{C} \quad . \quad . \quad . \quad . \quad . \qquad (24)$$

Thus the complete formula for ω is

$$\omega=\frac{\theta}{\left\{4\pi n-(\Sigma(\mathrm{A}))\left(\frac{1}{\mathrm{LA}\,.\,\mathrm{LB}}+\frac{1}{\mathrm{MA}\,.\,\mathrm{MB}}\right)\right\}\frac{\mathrm{H}r^3\tan\delta}{\Sigma(\mathrm{A})}}. \quad . \quad . \quad . \quad . \quad . \qquad (25)$$

The following quantities were taken by measurement:—



AB=26·34 centims.
LA=18·95 „
BM=23·72 „

Distance from O *to centre of helix*=141·64 centims.; whence

$$\mathrm{OA}=\mathrm{OB}=r=\left\{\overline{141\cdot64}^2+\overline{13\cdot17}^2\right\}^{\frac{1}{2}},$$

or

$$r^3=2878499, \text{ and } \log r^3=6\cdot4591660.$$

---

[*] This formula is due to Professor MAXWELL.

Tan δ.

The scale, which consisted of a wooden board on which was pasted a printed paper scale of millims., was fixed on a massive wooden tripod-stand fixed to the wooden floor. The telescope was, as before mentioned, bedded with plaster of Paris in a block of stone cemented on to a brick pier. [N.B. All the brick piers and tables were built on the solid ground and came up through holes in the wooden floor.] The scale was so adjusted that, when no current was passing, the line from O to the figure seen in the telescope was perpendicular to the scale. This adjustment was made by hanging a plumb-line near the scale, between it and the mirror O, so that on looking into the telescope it was seen in the centre of the field. A second was then hung in the line between the first and the mirror. The edge of a T square being placed so as just to touch both lines, its base gave the direction of the scale.

As it was not certain whether or not the paper scale had stretched in the pasting, the deflections were determined by taking, with a pair of compasses, the distance between the zero reading and that corresponding to any current, and measuring it on the standard brass scale. The numbers thus obtained are hereafter spoken of as "corrected deflections."

The readings were taken in each direction. Their mean was called x.

The distance from O to the mean zero-point was called a, and by measurement we have

$$a=102\cdot50 \text{ centims.}$$

The values of $y=\tan\delta$, where δ is the deflection of the suspended magnet, were obtained by the following formula, due to Professor MAXWELL. We have

$$\begin{aligned} y\equiv\tan\delta&=\tan\left(\tfrac{1}{2}\tan^{-1}\frac{x}{a}\right)\\ &=\sqrt{1+\frac{a^2}{x^2}}-\frac{a}{x}\\ &=\tfrac{1}{2}\frac{x}{a}-\tfrac{1}{8}\frac{x^3}{a^3}-\&\text{c.}^{*} \quad \ldots\ldots\ldots \quad (26) \end{aligned}$$

Determination of the Meridian.

The best way to have determined the meridian of the helix would have been to have deduced it from that of the suspended mirror. Owing, however, to the arrangement of the supports, this meridian could not be transferred to other parts of the table. The meridian was therefore determined by means of a needle furnished with a straw pointer 20 centims. long (by ELLIOTT), belonging to a large tangent galvanometer. Part of a

---

[*] This is a very simple formula to use, as we have

$$y=\tfrac{1}{2}\log^{-1}(\log x-\log a)-\tfrac{1}{8}\log^{-1}3(\log x-\log a).$$

sewing-needle was set upright in a hole drilled in the table, and filled with soft clay, at such a height that the magnetic needle when balanced on it just swung clear.

The latter was allowed to come to rest under a bell-glass, and then clamped by a simple arrangement. As the ends of the straw were 5 or 6 millims. from the table, a candle was placed close to the centre of the needle, and the ends of the shadow of the straw marked with a pencil. This meridian was then transferred to any other part of the table by means of parallel rulers*.

The following method was used for placing the helix at a known distance from the suspended magnet and perpendicular to the magnetic meridian, and for making that meridian which passed through the suspended magnet bisect the axis of the helix:—

A pencil line longer than the helix was drawn through the point on the meridian selected for the centre of the helix and perpendicular to that meridian. The plumb-bobs were hung at points, one near each end of this line and in it, and two more in the meridian passing through the suspended magnet.

A mark was made at the centre on the outside of the helix, and cross-wires placed at its ends inside. When the threads of the E.W. plumb-bobs sighted the cross-wires and the meridian-threads the point on its outside, the helix was properly placed.

Verification of the Meridian.

In order to see that there had been no error, the following verification was made, based on the principle that if the helix was placed symmetrically with regard to the meridian, the deflections produced in opposite directions by reversing the same current should be equal.

Readings.

Current direct.	No current.	Current reversed.
8·47	. . .	. . .
. . .	. . .	42·17
8·54	. . .	. . .
. . .	. . .	42·11
. . .	25·48	. . .

Corrected Deflections.

16·84	. . .	16·64

A difference too small to be worth correcting†.

The Light.

It was absolutely necessary that the light should be monochromatic, as the rotation of different colours for the same difference of magnetic potential is very different, varying nearly as λ^{-2}. A sodium flame was first tried but found not to be sufficiently steady; so it was decided to use a white light and a bisulphide-of-carbon prism. A very powerful paraffin-lamp with a duplex burner was finally chosen as the source of light. It was used with a lens and a slit, and threw a brilliant spectrum about 8 centims. long on a card. In the card was a vertical slit which admitted the light into the Nicol. By moving the prism, or lens, the spectrum could be moved along the card, and any desired colour admitted through the slit into the Nicol. In order to localize the light the lamp was removed from the lantern, and a spirit-lamp put in its place. The flame was coloured with thallium, and the green line projected on the card so as to fall upon the slit in it at minimum deviation. The lantern, lens, and prism being then clamped, the lamp was replaced, and the green light which fell upon the slit was of the same wave-length as that giving the thallium line. It was intended to make also observations at the sodium and lithium lines, but it was found impossible to isolate the sodium light, while to isolate the lithium the slits had to be made so narrow that the intensity was not sufficient to work by.

---

* See verification of meridian below.

† Professor MAXWELL has since pointed out to me that a slight displacement of the helix would not make so large a difference in the deflections as I had expected. I, however, leave in this paragraph for what it is worth.

In employing the Jellett analyzer, if the light used was not *perfectly* monochromatic, the effect of the magnetic force was to colour the two halves of the field different colours, rendering all comparison of intensities impossible. After a great number of preliminary observations (continued for about seven months), all of which were rejected as I found means to improve the methods used, the following sets were made.

In these sets no readings were rejected, except on one occasion, when, finding the first few readings did not agree well with one another, I rejected the whole of them, and after a short rest began them again from the beginning.

In all the observations the Nicol was so placed that the division line of the "Jellett" was about horizontal, so that if there were any difference in the light admitted at the right and left of the field it should affect both halves equally.

Thus the light on leaving the lamp passed through the apparatus in the following order:—slit, lens, prism, slit, Nicol, collimator, tube, Jellett and its mountings.

"Current direct" is that direction of current which causes the north-pointing end of the suspended magnet to move toward the east.

After each reading, the circle was slightly displaced at random.

Set 1. February 25.—11 to 11.30 P.M. Green light $\lambda=(5\cdot349)10^{-5}$ centims. 7 cells.

Plane of polarization.	Scale-readings.	Corrected deflections.
No current.	25·45	
Current direct.		
266° 12′ 0″		
266 12 0		
*266 17 0		
....	.... 3·71	21·63 centims.
266 16 0		
Mean 266 14 15†		
Current reversed.		
281 35 0		
281 36 0		
281 44 0		
....	.... 46·82	21·42 centims.
281 46 0		
Mean 281 40 15		

$2R=15^\circ\ 26'\ 0''$ $a=102\cdot5$ centims.

$x=$ mean of deflections $=21\cdot525$ centims.

$$\frac{2R}{y}=8917'\cdot5 \qquad y=\tan\delta=\tfrac{1}{2}\frac{x}{a}-\tfrac{1}{8}\frac{x^3}{a^3}=\cdot10384$$

Set 2. February 25.—11.30 to 12 P.M. Same light. 5 cells.

Plane of polarization.	Scale-readings.	Corrected deflections.
No current.	25·58	
Current direct.		
268° 6′ 0″		
268 5 0		
268 11 0		
....	.... 8·58	16·97
268 6 0		
Mean 267 7 0		
Current reversed.		
280 11 0		
280 9 0		
280 1 0		
....	.... 42·06	16·50
280 1 0		
Mean 280 5 30		

$2R=11^\circ\ 58'\ 30''$ $a=102\cdot5$ centims.

$x=16\cdot735$ centims.

$$\frac{2R}{y}=8861'\cdot1 \qquad y=\cdot081090$$

---

* With regard to the differences between different determinations of the plane of polarization, it must be remembered that it is impossible to keep the battery-current absolutely constant during a series of experiments extending over 10 or 12 minutes.

† The seconds only appear in the means.

Set 3. February 26.—0 to 0.30 A.M. Same light. 6 cells.

Plane of polarization.	Scale-readings.	Corrected deflections.
No current.	25·51	
Current direct.		
267° 17′ 0″		
267 26 0		
267 26 0		
	6·40	19·09 centims.
267 19 0		
Mean 267 22 0		
Current reversed.		
280 57 0		
281 5 0		
280 59 0		
	44·00	18·48 centims.
281 5 0		
Mean 281 1 30		

$2R = 13^\circ\ 39'\ 30''$ $a = 102{\cdot}5$ centims.

$x = 18{\cdot}785$ centims.

$\frac{2R}{y} = 8820'{\cdot}6$ $y = {\cdot}092907$

Tabulating the results, we have for $\frac{2R}{y}$

Set.	2R	$\frac{2R}{y}$	Cells.
2.	11° 58′ 30″	8861·1 minutes.	5
3.	13 39 30	8820·6 „	6
1.	15 26 0	8917·5 „	7

See p. 32.

To reduce the result to the D line, we may use one of the formulæ given by M. VERDET*; but as they are only approximate, I have thought it best to state the final result for the thallium line, wave-length

$$(5{\cdot}349)10^{-5}\dagger,$$

on which the experiments were made. The wave length of the D line is

$$(5{\cdot}892)10^{-5}\dagger.$$

Value of H.

The ratio of H at Dorking to H at Kew was determined by vibrating the same magnet at the two places, and determining the times of vibration. We then know that the values are inversely as the squares of the times of vibration.

Mr. WHIPPLE, Director of the Magnetic Observatory at Kew, had the great kindness, not only to lend me a magnet, but also to have it vibrated at Kew, and to determine the temperature coefficient and the correction for torsion of the thread.

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* Œuvres, tom. i. p. 211.

† WATTS' Index of Spectra.

The magnet was, at Pixholme, suspended to the same stone support and observed with the same telescope and scale as the smaller magnet used to determine the strength of the currents. It was therefore in exactly the same place as that magnet had been.

The method used for taking the time of vibration was explained to the author by Prof. MAXWELL, in a letter from which the following paragraphs are extracted*:—

"The best way to take the times is to begin with getting the approximate time of an odd number of half-vibrations 1, 3, or 5, according to convenience. Take such an odd number as will have a period of nearly half a minute or 20″, or something easily reckoned, then note down the time of passage through a point near the point of rest, marking it + when it goes to right, and − when it goes to left, and observe, say, 11 passages, 6+ and 5−, at convenient intervals. Then after a time equal to that of 100 vibrations or near it, begin again and observe 11 passages.

"From the two pairs of 11 passages find the time of vibration roughly, and thence the number of passages between the first and second set. Finally, find the time by dividing the mean interval between the first and second set by the number of passages. By taking the passages alternately + and −, you eliminate the error arising from taking the passage at a wrong point of the swing."

A large clock beating seconds was so placed that the observer at the telescope could see it by merely looking round. The times of passage were taken as follows:—

The hour and minute having been written down by an assistant, the observer noted the second, and went on counting the beats by ear. Then looking into the telescope, he observed the time of every 7th passage, and called it out to be written down by the assistant. The passages were thus always alternately + and −. The time occupied by 7 passages was sufficient to allow of the clock being looked at to get a fresh beat between each.

The approximate times at which each passage might be expected, within, say, 3″, were previously written out and placed in a convenient position. This prevented the possibility of a 5th or 9th passage being observed instead of a 7th. A thermometer hung in the case (wood with glass front) in which the magnet swung gave the temperature.

The following Tables give the results of the experiments—that of "Vibrations at Kew" being sent by Mr. WHIPPLE, and those of "Vibrations at Pixholme" being determined by the author.

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* See also MAXWELL'S 'Electricity,' Art. 456, vol. ii. p. 102.

Kew Observatory.

Observations of Vibration of Magnet $n\psi$ lent to Mr. J. E. H. Gordon.

Date.	Number of Observations.	Time of commencement.	Mean time of one vibration.	Torsion of thread $1+\frac{r}{H_m}$*.	Mean Temperature.
1876.		h m s	sec.		°
April 11......	1.	2 43 33	3·6880	1·00061	51·4
„ 11......	2.	3 28 41	3·6868	1·00067	52·8
„ 11......	3.	4 1 1	3·6866	1·00067	52·7
„ 13......	4.	11 6 29	3·6850	1·00069	40·3
„ 13......	5.	11 34 26	3·6858	1·00069	42·4
„ 13......	6.	11 49 37	3·6859	1·00069	43·2

$q=0·000178$.

$q'=0·000000596$.

q_0=the correction for the decrease of the magnetic moment of the magnet produced by an increase of temperature, and is expressed by a formula of the form—

$$\text{correction to } t_0=q(t_0-t)+q'(t_0-t)^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

t_0 being the observed temperature, and t an adopted standard temperature.

Vibration Experiments at Pixholme.

From some preliminary experiments which need not be detailed it was found that the time of a half-vibration at Pixholme was approximately 3·693 seconds.

The following Table gives the time of every 7th passage taking 6+ and 5−.

Set 1 (A).

April 23, 1876, P.M. Mean temperature 13°·9 C.

	Passages in + direction.	In − direction.
	h m s	h m s
	6 22 3½	6 22 29
	6 22 55	6 30 20½
	6 23 46½	6 24 12½
	6 24 38¼	6 25 4
	6 25 30	6 25 56
	6 26 21½	
Mean . .	6 24 12·458	6 24 12·400

Time of middle negative passage:—

	h m s
From + observations	6 24 12·458
From − „	6 24 12·400
Mean	6 24 12·429

---

* I have here altered the Kew notation to suit the letters commonly used in the C.G.S. system.

Set 1 (B).

	Passages in + direction. h m s	In − direction. h m s
	6 39 2	6 39 28
	6 39 53½	6 40 19½
	6 40 45¼	6 41 11⅓
	6 41 37	6 42 3
	6 42 28½	6 42 54½
	6 43 20¼	
Mean . .	6 41 11·83	6 41 11·26

Time of middle negative passage:—

	h m s
From + observations	6 41 11·83
From − „	6 41 11·26
Mean	6 41 11·545

Interval between mean negative passages A and B, 16 min. 59·251 sec.

If we divide by 3·693 the approximate period of a half-vibration, the nearest even whole number to the quotient will be the number of passages in the time.

Dividing by 3·693 we obtain the quotient 275·99, which gives 276 half-vibrations.

Dividing 1019·25 sec. by 276 we obtain for the time of one half-vibration 3·6929 seconds.

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Set 2 (A).

April 25, A.M. Mean temperature 11°·9 C.

	Passages in + direction. h m s	In − direction. h m s
	9 53 3	9 53 28⅓
	9 53 54¼	9 54 20
	9 54 46¼	9 55 12
	9 55 38	9 56 4
	9 56 29½	9 56 55½
	9 57 21¼	
Mean . .	9 55 12·0416	9 55 11·9666

Time of middle negative passage:—

	h m s
From + observations . .	9 55 12·0416
From − „ . .	9 55 11·9666
Mean . . .	9 55 12·0042

Set 2 (B).

	Passages in + direction.			In − direction.		
	h	m	s	h	m	s
	10	9	3½	10	9	29⅓
	10	9	55¼	10	10	21
	10	10	47	10	11	13
	10	11	38½	10	12	4½
	10	12	30¼	10	12	56
	10	13	22			
Mean . .	10	11	12·750	10	11	12·766

Time of middle negative passage:—

	h	m	s
From + observations . .	10	11	12·750
From − „ . .	10	11	12·766
Mean	10	11	12·7833

Interval between mean negative passages A and B, 16 min. 0·7791 sec.

Number of passages =260·1, that is 260.
Time=3·6953 sec.

Set 3 (A).

April 28, A.M. Mean temperature 11°·5 C.

	Passages in + direction.			In − direction.		
	h	m	s	h	m	s
	10	34	0	10	34	25½
	10	34	51½	10	35	17¼
	10	35	43⅓	10	36	9¼
	10	36	35	10	37	1
	10	37	27	[10	37	52¾]*
	10	38	18½			
Mean. .	10	36	9·222	10	36	9·150

Time of middle negative passage:—

	h	m	s
From + observations	10	36	9·222
From − „	10	36	9·150
Mean	10	36	9·186

---

* Interpolated.

Set 3 (B).

	Passages in + direction.			In − direction.		
	h	m	s	h	m	s
	10	48	2	10	48	28
	10	48	55	10	49	20
	10	49	46	10	50	11½
	10	50	37½	10	51	3¼
	10	51	29	10	51	55
	10	52	21			
Mean. .	10	50	11·750	10	50	11·550

Time of middle negative passage:—

	h	m	s
From + observations	10	50	11·750
From − „	10	50	11·550
Mean	10	50	11·650

Interval between mean negative passages A and B, 14 min. 2·564 sec.

Number of passages	228·1=228.
Time	3·6954 sec.

Set 4 (A).

April 28, P.M. Mean temperature 12°·9 C.

	Passages in + direction.			In − direction.		
	h	m	s	h	m	s
	2	1	5	2	1	30½
	2	1	56⅓	2	2	22
	2	2	48	2	3	14
	2	3	40	2	4	5½
	2	4	31½	2	4	57¼
	2	5	23.			
Mean . .	2	3	13·972	2	3	13·850

Time of middle negative passage:—

	h	m	s
From + observations	2	3	13·972
From − „	2	3	13·850
Mean	2	3	13·911

Set 4 (B).

	Passages in + direction.	In − direction.
	h m s	h m s
	2 12 2	2 13 28
	2 12 54	2 13 19½
	2 13 45½	2 14 11⅓
	2 14 37	2 15 3
	2 15 29	2 15 55
	2 16 20½	
Mean . .	2 14 11·333	2 14 11·366

Time of middle negative passage:—

	h m s
From + observations	2 14 11·333
From − „	2 14 11·366
Mean	2 14 11·350

Interval between mean negative passages A and B, 10 min. 57·439 sec.

Number of passages	178·0
Time	3·6935 sec.

Set 5 (A).

April 28, P.M. Mean temperature 13°·6 C.

	Passages in + direction.	In − direction.
	h m s	h m
	5 45 1¼	5 45 27
	5 45 53	5 46 19
	5 46 44½	5 47 10¼
	5 47 36¼	5 48 2
	5 48 28	5 48 54
	5 49 20	
Mean . .	5 47 10·500	5 47 10·450

Time of middle negative passage:—

	h m s
From + observations	5 47 10·500
From − „	5 47 10·450
Mean	5 47 10·4750

Set 5 (B).

	Passages in + direction.	In − direction.
	h m s	h m s
	5 55 59	5 56 24¼
	5 56 50¼	5 57 16¼
	5 57 42	5 58 8
	5 58 34	5 59 0
	5 59 25½	5 59 51¼
	6 0 17¼	
Mean . . .	5 58 8·00	5 58 7·97

Time of middle negative passage:—

	h m s
From + observations	5 58 8·00
From − „	5 58 7·97
Mean	5 58 7·985

Interval between mean negative passages A and B, 10 min. 57·510 sec.

Number of passages	178
Time	3·6938 sec.

CORRECTIONS TO VIBRATION TIME.

Temperature Correction.

The Kew formula was used, and as the coefficients of the magnet had been determined at Kew for the Fahr. scale, that scale was used in the work.

The square of the time of vibration varies inversely as the magnetic moment. Hence if the temperature of the magnet be greater than the standard temperature, the observed value of the vibration time will be too great, and its square must be divided by $(1+q_0)$, where q_0 is the correction given by the Kew formula.

The temperatures at which the vibration observations were taken were:—

At Kew. No. Obs.	At Kew. Temp. F.	At Pixholme. No. Obs.	At Pixholme. Temp. F.
K 1. . . .	51°·4	P 1. . . .	57°·0
2. . . .	52·8	2. . . .	53·4
3. . . .	52·7	3. . . .	52·7
4. . . .	40·3	4. . . .	55·2
5. . . .	42·4	5. . . .	56·5
6. . . .	43·2		

Let t the adopted standard temperature be that of K (4), which, being the lowest in either set, will make all the terms positive.

The following are then the values of q_0 :—

$$\text{K (1)}, \quad q_0 = \cdot 000178(51^\circ\cdot 4 - 40^\circ\cdot 3) + \cdot 000000596(51^\circ\cdot 4 - 40^\circ\cdot 3)^2 = \cdot 0020492.$$

Similarly :—

K (2) . .	$q_0 = \cdot 0023181$	P (1) . .	$q_0 = \cdot 0029908$
K (3) . .	„ $= \cdot 0022988$	P (2) . .	„ $= \cdot 0023420$
K (4) . .	„ $= 0$ [standard]	P (3) . .	„ $= \cdot 0022988$
K (5) . .	„ $= \cdot 0003586$	P (4) . .	„ $= \cdot 0026654$
K (6) . .	„ $= \cdot 00051895$	P (5) . .	„ $= \cdot 0028942$

Torsion of the Suspending Thread.

We require the ratio of the force of torsion to the magnetic directive force.

The Kew formula is (changing the notation to suit the letters commonly used in the C.G.S. system)

$$\frac{\tau}{H_m} = \frac{u^\circ}{90^\circ - u^\circ}$$

where τ is the force of torsion and u° is the change of declination produced by a twist of 90° in the torsion-thread.

As the magnet was not suspended from a torsion-circle, the only twist that could be applied was $\pm n\,(360^\circ)$ given by twisting the magnet completely round in a $\mp$ direction.

If, however, which for so small a correction as this is we may do, we assume that the change of declination produced by a twist of 360° is 4 times that produced by 90°, the formula still holds, only we must substitute 360° for 90° in the denominator; we have then

$$\frac{\tau}{H_m} = \frac{u^\circ}{360^\circ - u^\circ}, \quad \ldots\ldots\ldots\ldots \quad (28)$$

where u° is now the torsion produced by 360° of twist.

The following observations were made, the first by reading the scale when the magnet was at rest, the others by taking the limits of swing after stopping the magnet as nearly as possible by means of a magnetized penknife :—

Obs.		Scale-readings in centims.				Mean.
1.	No torsion	16·23				16·23
2.	Twist of 360°+	13·28 15·88	13·36 15·85	13·40 15·81	13·40 15·80	
		14·580	14·605	14·605	14·600	14·5975
3.	No torsion, viz. since (2) magnet turned 360°(−)	14·70 17·90	14·70 17·88	14·75 17·80	14·75 17·80	
		16·30	16·29	16·275	16·275	16·2850
4.	Twist of 360°(−).........	15·48 20·08	15·50 20·04	15·52 20·00	15·53 20·00	
		17·780	17·770	17·776	17·765	17·7687
5.	No torsion, viz. since (4) magnet turned 360°+	13·70 19·20	13·82 19·10	13·90 19·10	13·92 19·04	
		16·49	16·46	16·50	16·48	16·4825

Mean zero from (1) and (3)=16·2575. From (2) this gives that 360° of twist+ causes a deflection corresponding to 1·660 centim. on the scale.

Mean zero from (3) and (5)=16·3837, which from (4) gives a corresponding deflection of 1·385 centim.

Mean deflection therefore =1·5225 centim.

To obtain the number of minutes corresponding to this, we have the formula of (26), p. 16,

$$\tan u = \tfrac{1}{2}\frac{x}{a} - \tfrac{1}{8}\frac{x^3}{a^3},$$

where $a=102{\cdot}5$ centims., and $x=1{\cdot}5225$ centim.

This gives $\tan u = {\cdot}0074251,$

which from the tables gives the angle in minutes by which the declination is changed by a twist of 360°, viz.

$$u=25'{\cdot}46\,;$$

taking $\frac{1}{4}$ of this for the torsion produced by 90° of twist we have, from the Kew tables,

$$u=6'{\cdot}365 \text{ corresponds to } 1+\frac{\tau}{\mathrm{H}_m}=1{\cdot}00118.$$

Clock Rate.

The Kew observations are corrected for clock rate.

Owing to an accident to the comparing chronometer, the rate of the Pixholme clock was not determined till two or three days after the conclusion of the vibration experiments.

The Kew formula of correction is

$$\text{Real time of vibration}=(\text{observed time})\left(1-\frac{s}{86400}\right) \quad . \quad . \quad . \quad . \quad (29)$$

where s is the daily rate in seconds, being $+$ when clock gains, $(-)$ when it loses.

On April 29, at 4 P.M. the clock was 7 minutes 5 seconds slow*; on May 3, at 4 P.M. the clock was 9 minutes 30 seconds slow. Giving a rate of

$$s=-36{\cdot}25\,;$$

viz. it lost 36¼ seconds per day.

By interpolation from the Kew Tables we have

$$\left(1-\frac{s}{86400}\right)=1{\cdot}0004225$$

To obtain the times of vibration at 40°·3 we have

$$\text{Log (vibration time at } 40^{\circ}{\cdot}3)^2=2 \text{ log (observed time)}-\text{log } (1+q_0). \quad . \quad (30)$$

Also the torsion causes an apparent increase of magnetic force, and therefore decreases the time of vibration, whence

$$\text{Log (vibration time corrected for torsion)}^2=2 \text{ log (observed time)}-\text{log}\left(1+\frac{\tau}{\text{H}_m}\right). \quad (31)$$

Also

$$\text{Log (vibration time corrected for clock error)}=\text{log (observed time)}+\text{log}\left(1+\frac{s}{86400}\right). \quad (32)$$

Combining these three formulæ, we have for the true time of vibration corrected for temperature, torsion, and clock—

$$\text{Log (true vibration time)}^2=2\left(\text{log (observed time)}+\text{log}\left(1-\frac{s}{86400}\right)\right)$$
$$-\text{log}\,(1+q_0)-\text{log}\left(1+\frac{\tau}{\text{H}_m}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

We have the following values for the corrected vibration times:—

At Kew.

Set.	Log of observed time of vibration.	Log $(1+q_0)$.	Log $\left(1+\frac{\tau}{\text{H}_m}\right)$.	Log of square of true time of vibration.
1.	0·5667909	0·0008894	0·0002647	1·1324277
2.	0·5666496	0·0010057	0·0002909	1·1320026
3.	0·5666260	0·0009971	0·0002909	1·1319649
4.	0·5664375	0·0000000	0·0002996	1·1325754
5.	0·5665318	0·0001557	0·0002996	1·1326083
6.	0·5665435	0·0002253	0·0002996	1·1325621

---

* These observations were taken by the Dorking clock-maker.

At Pixholme.

Set.	$\text{Log}\left(1+\frac{\tau}{H_m}\right)=0{\cdot}0005122.$ $\text{Log}\left(1-\frac{s}{86400}\right)=0{\cdot}0001835.$		
	Log of observed time of vibration.	Log $(1+g_0)$.	Log of square of true time of vibration.
1.	0·5673675	0·0003003	1·1342895
2.	0·5676497	0·0010159	1·1341383
3.	0·5676615	0·0009971	1·1341807
4.	0·5674381	0·0001559	1·1345751
5.	0·5674734	0·0002550	1·1345466

From these data we get the ratio of the magnetic force at Pixholme to that at Kew, for

$$\frac{\text{H at Pixholme}}{\text{H at Kew}}=\frac{(\text{H at Kew at time of Kew vibrations})\quad.\quad(\text{Vibration time at Kew})^2}{(\text{H at Kew at time of Pixholme vibrations})\;.\;(\text{Vibration time at Pixholme})^2}\qquad(34)$$

and the values of H at Pixholme at the times of the optical experiments equal the values at Kew at those times multiplied by the above ratio.

The last columns of the two following Tables supply respectively the numerator and denominator of the above fraction.

Set.	Dates of vibration experiments at Kew.	Mean value of H at Kew	(Corrected square of vibration time at Kew) × (Mean value of H at Kew)
		at these dates.	
1.	1876. April 11, 3 P.M.	0·179109	2·42964*
2.	" " 3.30 P.M.	0·179132	2·42759
3.	" " 4.0 P.M.	0·179146	2·42758
4.	April 13, 11 A.M.	0·178898	2·42768
5.	" " 11.30 A.M.	0·178856	2·42618
6.	" " Noon	0·178866	2·42711
		Mean	2·42723

* Rejected.

Set.	Dates of vibration experiments at Pixholme.	Mean value of H at Kew	(Corrected square of vibration time at Pixholme) × (Mean value of H at Kew)
		at these dates.	
1.	1876. April 23, 6.30 P.M.	0·179368	2·44362
2.	April 25, 10 A.M.	0·178935	2·43688*
3.	April 28, 10.30 to 10.50 A.M.	0·179024	2·44395
4.	„ „ 2.2 to 2.15 P.M.	0·179190	2·44281
5.	„ „ 5.45 to 6 P.M.	0·179245	2·44339
		Mean	2·44344

This gives

H at Pixholme =(·993366) H at Kew.

But at the dates of the optical experiments H had, at Kew, the values given in the second column of the following Table, and therefore at Pixholme those in the third.

Set.	Dates of optical experiments at Pixholme, 1876.	Mean values of H at Kew	Mean values of H at Pixholme
		at these dates.	
1.	Feb. 25, 11 to 11.30 P.M.	0·179117	0·177929
2.	Feb. 25, 11.30 P.M. to Midnight	0·179241	0·178051
3.	Feb. 25, Midnight, to Feb. 26, 0.30 A.M.	0·179244	0·178055

Dividing the three values of $\frac{2R}{y}$ given on page 20 by these numbers respectively, we have, for the three quantities which ought to be identical,

$$\begin{aligned} \frac{2R}{yH} = & (1) \quad 50118{\cdot}4 \\ & (2) \quad 49767{\cdot}0 \\ & (3) \quad \underline{49538{\cdot}7} \\ \text{Mean} \ . \ . \ . \quad & \quad\quad\ 49808{\cdot}0 \end{aligned}$$

Extreme difference from the mean 0·6 per cent.

We have, finally, then, if ω be the rotation in bisulphide of carbon of the plane of polarization of the ray whose wave-length is

$$(5{\cdot}349)10^{-5}$$

---

* Rejected.

between two points whose magnetic potentials differ by unity,

$$\omega=\frac{\frac{k2\mathrm{R}}{\tan\delta}\cdot\frac{1}{\mathrm{H}}\cdot\Sigma(\mathrm{A})}{r^3\left\{4\pi n-\Sigma(\mathrm{A})\left(\frac{1}{\mathrm{LA}\,.\,\mathrm{LB}}+\frac{1}{\mathrm{MA}\,.\,\mathrm{MB}}\right)\right\}},\quad \ldots\ldots\ldots \quad (35)$$

where k is the number of units of circular measure in a minute of arc $=\frac{3\cdot14159}{180\times60}$;

or $$\omega=\frac{(\cdot00029088)(77453)}{2878499\left\{4\pi(1028\cdot15)-77453\left(\frac{1}{(18\cdot95)(45\cdot29)}+\frac{1}{(50\cdot06)(23\cdot72)}\right)\right\}}\cdot\frac{2\mathrm{R}}{\tan\delta\,.\,\mathrm{H}}$$

$$=(\log^{-1}\overline{10}\cdot7866630)(49808\cdot0)$$

$$=\log^{-1}\bar{5}\cdot4839621$$

$$=3\cdot04763(10^{-5}).$$

As the constant is the ratio of a number to a current, its dimensions are

$$[\omega]=[\mathrm{M}^{-\frac{1}{2}}\mathrm{L}^{-\frac{1}{2}}\mathrm{T}].$$

This is VERDET's constant in absolute measure. For light of a given wave-length passing through a given substance, it is a fixed and definite physical quantity, depending only on the units of length, mass, and time*.

It is the number in optical measure which is equivalent to unity in electro-magnetic measure. In future investigations quantities expressed in electro-magnetic measure can be expressed in optical measure by multiplying them by this number.

ω, then, is defined to be VERDET's constant for the thallium ray in bisulphide of carbon, expressed in C.G.S. measure.

I here insert, as I have been requested to do, the result of my former paper on the same subject. It is that for distilled water with white light,

$$\omega=4\cdot496(10^{-6})\dagger.$$

I do not, however, attach much value to the result, as the different determinations are in the ratios of

7·563, 7·406, 8·295, 8·401, 6·916,

showing variations from the mean of ±7 per cent., or giving only about $\frac{1}{10}$ of the accuracy of the present paper.

The methods used for determining the constants were also susceptible of less accuracy.

Before concluding this paper, I must express my thanks, first and chiefly, to Professor MAXWELL, who has superintended every detail of the work for the year and eight months during which it has been in progress; and secondly, to Mr. Whipple, Director of the Kew Magnetic Observatory, who sent me all the data for calculating the value of H, and had several series of experiments and calculations made for me at the Observatory.

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* The magnetic rotative power of bisulphide of carbon here comes in the same way as the specific heat of water comes into JOULE's equivalent.

† By an error in arithmetic this was printed 10^{-7} in the abstract of my paper published Proc. Roy. Soc. June 1875.

Appendix.

Analysis of the Bisulphide of Carbon employed.—Mr. J. M. Thomson, Demonstrator of Chemistry at King's College, London, had the kindness to test the purity of the bisulphide for me. He writes:—"As far as I can see, the CS_2 you sent me is perfectly good. Its sp. gr. (the mean of three determinations) is 1·275 at 15°, the sp. gr. of pure CS_2 being 1·271 at 15°. Its boiling point is rather above 47°, so that the sample may be considered as very nearly pure. It leaves a trace of sulphur on evaporation, but not a quantity that can be weighed."

During the year which elapsed between the experiments and the testing of the bisulphide the latter was kept in a stoppered bottle of thick blue glass.

The temperature of the bisulphide, at the time of the optical experiments, was unfortunately not taken; but I believe it was about 55° F., as it was a frosty night, while a good fire kept the laboratory pleasantly warm.

The logarithms used in the calculations are from Chambers's Mathematical Tables, Ed. 1873.

II. *The Calculus of Chemical Operations; being a Method for the Investigation, by means of Symbols, of the Laws of the Distribution of Weight in Chemical Change.*—Part II. *On the Analysis of Chemical Events.* *By Sir* B. C. Brodie, *Bart., F.R.S., late Professor of Chemistry in the University of Oxford.*

Received January 13,—Read May 18, 1876.

> "*The Observer is not he who merely sees the thing which is before his eyes, but he who sees what parts that thing is composed of.*"—J. Stuart Mill.

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Introduction.—ON THE LAW OF EVEN NUMBERS.

The first part of this Calculus was devoted to the construction of those rudimentary tools of analytical investigation termed Chemical Symbols. I have there given expression, by a system of arbitrary signs, to certain mental conceptions and combinations of conceptions which enter into exact chemical inquiries. This involves an analysis of those conceptions. But something more, too, is there effected. For the study of these questions necessitates the reconstruction (to a certain extent) of the fundamental ideas of the science and, especially, the reconsideration of a problem supposed to have been, long since, finally determined, namely, the constitution of the units of ponderable matter, of which I have given a new theoretical analysis. I shall not attempt to give any summary of these results, which, in my previous Memoir, have been discussed as briefly as is consistent with clearness. At the same time I should observe that the following pages can only be intelligible to those who have already made themselves acquainted with the principles of this Calculus, and to such alone they are addressed. There is, however, a point of fundamental importance which as yet has been only incidentally touched, namely, the origin of the hypothesis that the unit of hydrogen is an "undistributed weight," which is the keystone of the system here adopted, and the reasons by which that hypothesis is justified, on which it is desirable, before proceeding further, to offer a somewhat fuller explanation. The following slight outline of the treatment of the subject pursued in this Calculus is given merely with the view of introducing these questions.

For the effective consideration of the chemical properties of matter it is necessary to refer these properties to a common standard of comparison. Our first step, therefore, was the definition of the "Unit of ponderable matter" (I. Section I. (10)). In the selection of this unit we are guided by the same principles as those on which we select the unit of length, the unit of weight, the unit of heat, our choice being in all such cases determined by what is convenient for the special purpose in view. Now the objects of our study are the chemical nature and transformations of gaseous matter. If, therefore, we wish to reason with impartiality, we must compare the properties of equal volumes of gases existing under the same conditions of temperature and pressure, for no reason can be assigned for comparing unequal volumes. All gases must be treated alike.

We are thus led to the notion of the "unit of space," a peculiar and essential feature of this Calculus. This "unit of space" is the empty measure in which we measure out the "units of matter." The measure chosen is 1000 cub. centims.; and were we to measure out the volumes of gases in this standard measure at a temperature of 0° C. and a pressure of 760 millims., we should be in possession of the "units of matter." We shall consider this estimate to have been, in some way, made.

Here we are met by the preliminary difficulty that this estimate involves numerous assumptions, so that our results are by no means conclusive. Even in the case of actual gases and vapours we cannot determine our units without, at least, assuming the truth of the law of MARIOTTE and of the recognized variation of volume due to changes of temperature. But the greater number of chemical substances do not exist for us as gases at all. Here we are driven to select as the "unit of matter" that quantity of matter which we infer, from probable reasoning of various kinds, would occupy our measure, in the gaseous state, under certain normal conditions, if only we could thus estimate it. Hence undoubtedly our results are liable to be affected with very serious errors. So that it may be truly said that the theory of chemistry refers rather to an ideal world created by the reason of man out of the actual world than to the actual world itself. There is, however, a wide interval between the ideal creations of the reason and the figments of the imagination. In the long run the former correspond to realities, and the comparison of the results as to the densities of gases which have actually been anticipated by this mode of probable reasoning with those subsequently arrived at by conclusive experiments prove us to be on the right track.

If it be asked why we do not, as is usually done, select a "molecule" as our unit, we reply, not that the molecular hypothesis is untrue (this would be going beyond the mark), but that it is unnecessary. We do not object to it, but we do not use it, for we do not want it for our present purpose. Having thus obtained the "unit of matter," the inquiry before us is, by what operations are these units of matter made? We begin by endeavouring to answer another question, namely, of what are they made? To this but one rational reply can be given. The units of matter are made of the matter of one another. But this is not all that can be said, for we may proceed to ask how, in particular cases, these units are thus made up.

The matter of two units of water, 2C, is made up of the matter of two units of hydrogen, 2A, and one unit of oxygen, D, and is therefore identical with that matter. We may state this identity in an equation, thus—

$$2C=2A+D;$$

whence

$$D=2C-2A,$$

$$=2(C-A).$$

Now we give a complete definition of the unit of oxygen, D, when we say that the matter of a unit of oxygen is identical with the matter of two units of water without the matter of two units of hydrogen; but we may further, from the last equation, assert that the matter of a unit of hydrogen, A, is a part of the matter of a unit of water, C. If, then, in the above equation, making this assumption, we put $C=B+A$, we have $D=2B$.

Further, assuming the matter of two units of water to be identical with the matter of a unit of hydrogen and the matter of a unit of binoxide of hydrogen*, and putting E as the matter of a unit of binoxide of hydrogen, we have

$$2C=A+E,$$

---

* The vapour-density of binoxide of hydrogen has not been experimentally determined. If this vapour-density should prove to be other than that here given, we should be compelled to take a different view of the constitution of water; but, from all we know, this is a highly improbable contingency.

whence, substituting for C the value $A+B$, we have

$$2A+2B=A+E;$$

and $E=A+2B$, giving, as the constituents of the unit of matter considered:—

Unit of Hydrogen	A	weighing	·089 grm.
„ Oxygen	$2B$	„	1·430 „
„ Water	$A+B$	„	·805 „
„ Binoxide of hydrogen	$A+2B$	„	1·520 „

We cannot, from the evidence before us, assign any constitution to the bits of matter A and B, nor are we able to make any assertion about these bits of matter, except that the bit of matter A weighs ·089 grm., and that the bit of matter B weighs ·715 grm.

A bit of matter as to which our knowledge is thus limited is what is here termed a "simple" or "undistributed weight." These terms do not refer to the constitution of the bits of matter A and B, as to which we have no means of forming an opinion, but to a very different thing, namely, our knowledge of that constitution, which is confined within narrow bounds determined by our powers of observation and experiment.

The groups of letters A, $2B$, $A+B$, $A+2B$, although truly symbols or signs of the matter of which the units of hydrogen, oxygen, water, and binoxide of hydrogen consist, are not the symbols of those units of matter themselves. A symbol must distinguish the thing symbolized from other things, but these expressions do not thus distinguish those units of matter. Thus the expression $A+2B$ indicates (to one who knows the meaning here given to the letters) the matter of which the unit of binoxide of hydrogen is constituted; but it indicates to him several other things besides, namely, the matter of a unit of hydrogen, A, and the matter of a unit of oxygen, 2B, and also the matter of a unit of water, $A+B$, and the matter of the simple weight B, and also the matter of the unit of hydrogen, A, and of two simple weights, B and B. So that when such an expression is presented to us (if no more be said) we cannot tell to which of these different objects the expression refers. The expression $A+2B$ indicates something common to all these objects, but does not indicate any one object specially. How, then, are we to frame such a distinctive symbol? This question has been fully considered in the first part of this Calculus, to which I must refer the reader for information. I will here only make one remark. The chemical symbol of a unit of matter, as constructed on the principles of this Calculus, is an analytical expression which indicates to one acquainted with those principles that special combination of operations by which, in the processes of chemical change, the unit has been made up. These operations are known to us only through their results, and are defined by those results; indeed no other knowledge of them is practicable; but this is sufficient for our purpose, which is the comparison of those results.

That operation which I have here termed a chemical operation, and defined as an operation performed upon the unit of space, of which the result is "a weight"

(I. Sec. II. (1)), is an operation with one variety of which chemists are familiar under the name "chemical combination." I have not employed this language, for the terms are by no means coextensive; and we should be led into unnecessary difficulties by restricting "a chemical operation" to that particular kind of "chemical operation" implied in the term "combination" with all the ideas which have grown up around it. If, however, we are willing to discard material images, and consider "combination" in a more general and abstract sense, the term will work. Let $x, y, z \ldots$ be the symbols of the operations of chemically combining the "weights" or bits of matter, A, B, C, . . ., at 0° and 750 millims. pressure, then $x, y, z \ldots$ are termed the chemical symbols of A, B, C . . . respectively at that temperature and pressure. Also let xy be the symbol of the "combination" of A and B, and $xyz \ldots$ the symbol of the "combination" of A and B and C . . ., then $xxx \ldots$ is the symbol of the "combination" of A and A and A . . . Further let z be the symbol of the "weight" or matter contained in an empty unit of space, then zz (or z^2) is the symbol of the combination of the "weights" (or matter) contained in two empty units of space, and z^p is the symbol of the combination of the "weights" in p empty units of space. But the "weights" in two empty units of space are collectively identical with the "weight" in one empty unit of space, being in both cases "no weight," and the weights in p empty units of space are collectively identical with the "weight" in one empty unit; we have, therefore, $z^p = z$, and generally $xy \ldots z^p = xy \ldots$ Now among the symbols of number we have one symbol, and one symbol alone, which satisfies the condition satisfied by the symbol z, namely, the symbol 1. If, therefore, we put 1 as the symbol of the "weight" contained in an empty unit of space, and work with this symbol (as a factor) in the algebra of chemistry, according to precisely the same rules as in general algebra we work with the numerical symbol 1, we shall never be led into error. My object in these remarks is to point out the intimate connexion which subsists between the principles of this Calculus and those fundamental ideas which have been developed by the requirements of the science[*]. I shall not pursue the subject, as it has been fully treated in Part I. Sec. III.

The symbols of the units of hydrogen, oxygen, water, and binoxide of hydrogen, as thus expressed, are:—

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[*] The objects of chemistry, considered as an art and as a science, were defined by the illustrious STAHL in the following words:—"Chymia alias Alchymia et Spagirica, est ars corpora vel mixta vel composita vel aggregata etiam in principia sua resolvendi aut ex principiis in talia combinandi.

"Subjectum ejus sunt omnia mixta et composita quæ resolubilia et combinabilia. Objectum est ipsa resolutio et combinatio, seu corruptio et generatio."—STAHL, *Fundamenta Chymiæ*, Norimbergæ MDCCXXIII.

The "resolubilia" and "combinabilia" of STAHL correspond to what is here termed ponderable matter [I. Sec. I. Def. (1)], while the "ipsa combinatio" and "ipsa corruptio" represent, in his order of ideas, what is here termed a "chemical operation" [I. Sec. II. Def. (1)].

Unit of Hydrogen	α,
„ Oxygen	ξ^2,
„ Water	$\alpha\xi$,
„ Binoxide of hydrogen . . .	$\alpha\xi^2$,

α being the symbol of the "weight" A, and ξ the symbol of the "weight" B.

These symbols involve no hypothesis whatever except the accuracy of the data from which they are deduced; and were we ignorant of the existence of the elements chlorine, iodine, bromine, nitrogen, phosphorus, . . ., and the other elements (hydrogen excepted) of the class termed by LAURENT "dyad elements"[*], it would be in our power, proceeding on these principles, to construct a perfectly unhypothetical symbolic system (which would be open to no difference of opinion) to represent the units of matter.

Now, however (retaining A as indicating the matter of a unit of hydrogen), let E be the matter of a unit of hydrochloric acid and C the matter of a unit of chlorine. We have then

$$2E=A+C,$$

and

$$C=2E-A;$$

the only inference to be necessarily drawn from this equation is that the matter of a unit of hydrogen is contained in (or is a part of) the matter of two units of hydrochloric acid.

Assuming each of the two units of hydrochloric acid to be similarly constituted (for we cannot draw any distinction between them without an assumption, which is totally unnecessary), this condition may be satisfied in two ways. I. A unit of hydrogen may be contained in one unit of hydrochloric acid. II. A unit of hydrogen may be contained in two units of hydrochloric acid and not contained in one unit of hydrochloric acid.

Considering Case I., let $E=A+K$, $C=A+2K$, and we have as the constituents of the units of hydrogen, chlorine, hydrochloric acid,

Unit of Hydrogen	A	weighing	·089 grm.,
„ Chlorine	$A+2K$	„	3·173 grms.,
„ Hydrochloric acid . . .	$A+K$	„	1·631 grm.,

the weight of the bit of matter K being 1·542 grm.

This view corresponds to the hypothesis that the unit of hydrogen is a simple weight.

Symbol of Hydrogen	α,
„ Chlorine	$\alpha\chi^2$,
„ Hydrochloric acid	$\alpha\chi$.

In Case II. half a unit of hydrogen (and no more or less) must be contained in a unit of hydrochloric acid. Putting, then, $E=\frac{A}{2}+D$, we have $C=2D$. The constituents of the several units being

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[*] LAURENT, 'Chemical Method,' Cavendish Society, 1855.

Unit of Hydrogen	$2\times\frac{A}{2}$,	weighing	0·089 grm.,
„ Chlorine	2D	„	3·173 grms.,
„ Hydrochloric acid . .	$\frac{A}{2}+D$	„	1·631 grm.,

the weight in grammes of the bits of matter $\frac{A}{2}$ and D being respectively 0·044 grm. and 1·586 grm. This result corresponds to the view now generally received by chemists as to the constitution of these units, according to which the matter of the unit of hydrogen is regarded as constituted of two bits of matter in all respects identical. Were we to construct the symbols of these units on the principles of this Calculus, we should arrive at the following system, which corresponds to the assumption that the unit of hydrogen is constituted of two simple weights:—

Symbol of the unit of Hydrogen	a^2,	weighing	0·089 grm.
„ „ Chlorine	c^2	„	3·173 grms.
„ „ Hydrochloric acid .	ac	„	1·631 grm.

It is important to observe that there are only two such symbolic systems*, which,

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* Besides the systems originating in these two hypotheses respectively, there is also the general or indeterminate system comprehending both, in which we have

	Symbol.
Hydrogen	$\alpha'\varpi'^2$
Chlorine	$\alpha'\chi'^2$
Hydrochloric acid	$\alpha'\chi'\varpi'$
Nitrogen	$\alpha'\nu'^2$
Ammonia	$\alpha'^2\nu'\varpi'^3$
. . . .	.
. . . .	.

If in this system of symbols we put $\varpi'=1$, it becomes the system on hypothesis α; if we put $\alpha'=1$, it becomes the system on hypothesis α^2. We may consider the system to originate in the following manner. Taking the equation

$$2E=A+C,$$

let X be the matter common to A and C, and Y and Z the other constituents of A and C respectively, so that

$$A=X+Y,$$
$$C=X+Z;$$

we have then

$$2E=(X+Y)+(X+Z)$$

and

$$E=X+\frac{Y+Z}{2}.$$

Since Y and Z have no common part, $\frac{Y}{2}$ must be a "simple weight," and $\frac{Z}{2}$ also a simple weight, whence we have the units of hydrogen, chlorine, and hydrochloric acid constituted as follows:—

without irrelevant assumptions, can possibly be constructed—the system to the base α[*], based on the hypothesis that in chemical transformations the unit of hydrogen is an "undistributed" or "simple weight," and the system to the base α^2, based on the assumption that the unit of hydrogen is constituted of two simple weights into which that unit is distributed in those transformations. These systems are mutually exclusive, and cannot both be true. Now we are not in a position to assert that the one of these systems is true and the other false. If so, one system would be applicable and the other inapplicable to the facts, and in the construction of our system we should get rid of hypothesis altogether. It is rarely, however, indeed that in any case such a scientific construction is practicable. But failing this, and admitting the inadequacy of our information, we may still ask, can we give a reasonable preference to the one or the other system? Even this might be out of our power. To what extent, too, is this preference to be carried? These are questions of probable reasoning which must be left to the judgment of individuals; but if we are to assume these questions to be decided before we begin to consider them, we may as well not consider them at all.

I may first observe that the two systems are not at the outset placed upon a precisely equal footing; for it is certain that in a very large proportion of chemical events the

---

Unit of Hydrogen . . . . . . $X+2\left(\frac{Y}{2}\right)$,

" Chlorine . . . . . . $X+2\left(\frac{Z}{2}\right)$,

" Hydrochloric acid . . . $X+\left(\frac{Y}{2}\right)+\left(\frac{Z}{2}\right)$;

whence, putting α' as the symbol of the simple weight X, ϖ' as the symbol of the simple weight $\frac{Y}{2}$, and χ' as the symbol of the simple weight $\frac{Z}{2}$, we arrive at the equation

$$2\alpha'\chi'\varpi'=\alpha'\varpi'^2+\alpha'\chi'^2.$$

This system was pointed out to me, at the time of the appearance of the first part of this memoir, by Professor G. Stokes; it was subsequently noticed by Professor Crum Brown ('Philosophical Magazine' for August 1867), and again, more recently, by Professor Clifford in a paper read before the British Association at the Meeting in Belfast, 1874.

This system contains one indeterminate symbol, that is to say, the symbol of one simple weight which cannot be determined from the data given in the equation. If we had before us merely the single equation just considered, we could not possibly say, without an arbitrary (and therefore unmeaning) assumption, to which of the two special systems it was to be referred; and the only rational course to pursue would be to express the result in the indeterminate system, thus keeping both hypotheses before us. But the case is different when we come to consider the expression of the total system of chemical equations, which afford us the means of selection with probability, although not with certainty, between the two determinate systems.

[*] In Part I. Sec. VII. (1) I have termed α the "modulus" of the symbolic system, it being that symbol by which the form of every other symbol of the system is regulated. As I have occasion to use the term "modulus" for another purpose, this use of it would give rise to ambiguity, and I shall substitute for it the term "base."

unit of hydrogen is not "distributed," and is necessarily (unless we make totally unmeaning assumptions) to be expressed by one prime factor a. If, therefore, we are to depart from this principle some ground must be shown for so doing.

The comparative value of two hypotheses may be tested by following these hypotheses out to their consequences, and comparing these consequences with the facts to which the hypotheses are to be applied. If we find that one hypothesis is applicable to all the facts, while the other is applicable to only a portion of those facts, the latter hypothesis is inadmissible. But both hypotheses may include all the facts, and be equally applicable to them, including in each case precisely the same facts. In this case both hypotheses are equally tenable, and we have no means of giving a preference to either. But there is another case: both hypotheses may include all the facts, and so far either may serve our purpose; but one of the two may also do more than we want, covering more ground than is required, and indicating improbable results. In this case that hypothesis is to be preferred the consequences of which most exactly coincide with the facts, namely, the more restricted hypothesis.

Assuming, then, that we are in possession of the symbols of the units of ponderable matter constructed, with logical precision, upon these two several hypotheses, let us consider the consequences to be deduced from the expressions thus assigned to them.

To simplify the problem, let us take the case of the compounds of hydrogen, chlorine, oxygen, carbon.

If m units of any chemical substance be made up of p units of hydrogen, p' units of chlorine, q units of oxygen, and r units of carbon, m, p, p', q, r being positive integers, what are the relations, if any, by which these integers are connected? We shall assume m, p, p', q, r to have no common measure.

I. We will first consider hypothesis a^2. On this system we have :—

Symbol of Hydrogen . . . . a^2,
" Chlorine . . . . . . c^2,
" Oxygen . . . . . . ξ^2,
" Carbon . . . . . . $\varkappa^t$,

the last two symbols being identical in the two systems. The symbol, then, of a unit of matter, expressed by the prime factors $a, c, \xi, \varkappa$, is $a^n c^{n'} \xi^{n''} \varkappa^{n'''}$ [I. Sec. V.], whence, from the conditions of the problem, we have

$$m\, a^n c^{n'} \xi^{n''} \varkappa^{n'''} = pa^2 + p'c^2 + q\xi^2 + r\varkappa^t;$$

whence [I. Sec. V. (10)]

$$nm = 2p,$$
$$n'm = 2p',$$

$$n''m=2q,$$

$$n'''m=\ tr,$$

and

$$n=\frac{2p}{m},\ \text{a positive integer,}$$

$$n'=\frac{2p'}{m}\qquad ,,\qquad ,,$$

$$n''=\frac{2q}{m}\qquad ,,\qquad ,,$$

$$n'''=\frac{tr}{m}\qquad ,,\qquad ,,$$

It appears from these equations that m is either a factor of p, p', and q, or is of the form $2m_1$, where m_1 is a factor of p, p', and q.

Further, since m is a factor of tr, if m be a factor of p, p', and q, m is prime to r, and m is a factor of t. If $m=2m_1$, since m_1 is prime to r, m_1 is a factor of t. If we assume, therefore, in conformity with the results arrived at as to the symbol of carbon (I. Sec. VII. (2)), $t=1$ and $t=2$ as the most probable assumptions in the case of that symbol, we have in the former case,

$$t=1,\ n'''=\frac{r}{m},\ \text{and}\ m=1,$$

in the latter case,

$$t=2,\ n'''=\frac{2r}{m};$$

whence either $m=1$ or is of the form $m=2m_1$, where m_1 is a factor of r. But m is prime to r; therefore we have on these assumptions only two values for m, $m=1$ or $m=2$. It is, however, to be borne in mind that this limitation of the value of m implies a more definite knowledge of the hypothetical density of carbon than we possess. These results are the only consequences to be here deduced from the joint hypotheses that the units of ponderable matter are made up of an integral number of simple weights, and that the unit of hydrogen is constituted of two identical simple weights. The application of these principles will be readily seen in a particular case. For example :—

One unit of trichloracetic acid weighing 7·308 grms. is identical with half a unit of hydrogen, three halves of a unit of chlorine, one unit of oxygen, and some unknown number of units of carbon (to be hypothetically determined) weighing 1·072 grm. Putting the symbol of carbon as x^t, where t is an undetermined integer, we will call this unknown number of units $\frac{\mathrm{R}}{m_1}$. Expressing this identity in an equation, we have

$$m_1\,ac^2\xi^2x^2=\frac{m_1}{2}\,a^2+\frac{3m_1}{2}\,c^2+m_1\xi^2+\mathrm{R}x^t$$

and

$$2m_1\, ac^2\, \xi^2\, \kappa^2 = m_1\, a^2 + 3m_1\, c^2 + 2m_1\, \xi^2 + 2\mathrm{R}\kappa^t.$$

Comparing this equation with the general equation given above, we perceive that it falls under the case $m=2m_1$, where m_1 is a factor of p and p' and q.

If we assume $t=1$, which corresponds to the assumption that the weight of a unit (that is, of 1000 cub. centims.) of carbon vapour at 0° and 760 millims. is 0·536, $\frac{\mathrm{R}}{m_1}=2$, R$=2$, and $m_1=1$, the equation being

$$2ac^2\xi^2\kappa^2 = a^2 + 3c^2 + 2\xi^2 + 4\kappa.$$

If we assume $t=2$, which corresponds to the assumption that the weight of a unit of carbon vapour is 1·072 [I. Sec. VII. Group 2, III.], $\frac{\mathrm{R}}{m_1}=1$, R$=1$, $m_1=1$, the equation being

$$2ac^2\, \xi^2\, \kappa^2 = a^2 + 3c^2 + 2\xi^2 + 2\kappa^2.$$

Again, one unit of dichloracetic acid is identical with a unit of hydrogen, a unit of chlorine, a unit of oxygen, and (as before) R units of carbon. In this case m is a factor of p, p', q, the equation being

$$m\, a^2\, c^2\, \xi^2\, \kappa^2 = m\, a^2 + m\, c^2 + m\, \xi^2 + \mathrm{R}\kappa^t.$$

There are two forms of this equation, corresponding to the hypotheses $t=1$, $t=2$:

$$t=1,\ a^2\, c^2\, \xi^2\, \kappa^2 = a^2 + c^2 + \xi^2 + 2\kappa,$$
$$t=2,\ a^2\, c^2\, \xi^2\, \kappa^2 = a^2 + c^2 + \xi^2 + \kappa^2.$$

No limitation whatever is imposed by this hypothesis on the value of n, n', n'', n'''. Two cases may be discriminated:

$$(1)\ m=1,\ n=2p,\ n'=2p',\ n''=2q,\ n'''=r,$$
$$(2)\ m=2,\ n=p,\ \ n'=p',\ \ n''=q,\ \ n'''=r;$$

it being always remembered that m, p, p', q, r have no common measure.

It hence appears that every combination of the letters a, c, ξ, κ of the form $a^n c^{n'} \xi^{n''} \kappa^{n'''}$ is the symbol of a unit of ponderable matter capable of being resolved into some number of units of hydrogen, chlorine, oxygen, and carbon, on the assumption here made as to the constitution of those elements.

Now the symbols of all known compounds of these elements are to be found among these combinations, with their density and composition correctly indicated to us. Hence the hypothesis that the symbol of hydrogen is a^2 covers the whole ground, and is sufficient for our purpose. If it be defective, it is not by reason of its insufficiency. But there are other points to be considered.

II. On the hypothesis α, namely, that the symbol of hydrogen is to be expressed by one prime factor, we have:—

Symbol of Hydrogen . . . . . α,
" Chlorine . . . . . . $\alpha\chi^2$,
" Oxygen . . . . ξ^2,
" Carbon . . . . κ^t,

the symbol of a unit of matter as expressed by the prime factors $\alpha, \chi, \xi, \kappa$ being $\alpha^n\chi^{n'}\xi^{n''}\kappa^{n'''}$.

From the conditions of the problem we have

$$m\alpha^n\chi^{n'}\xi^{n''}\kappa^{n'''}=p\alpha+p'\alpha\chi^2+q\xi^2+r\kappa^t;$$

whence

$$nm=p+p',$$
$$n'm=2p',$$
$$n''m=2q,$$
$$n'''m=tr,$$

and

$$n=\frac{p+p'}{m}, \text{ a positive integer,}$$
$$n'=\frac{2p'}{m} \quad \text{"} \quad \text{"}$$
$$n''=\frac{2q}{m} \quad \text{"} \quad \text{"}$$
$$n'''=\frac{tr}{m} \quad \text{"} \quad \text{"}$$

There are two cases.

I. m is a factor of p, p', q and is prime to r.

If $t=1$, $n'''=\frac{r}{m}$, and $m=1$, since p, p', q, r are so taken as to have no common measure, and

$$n=p+p',$$
$$n'=2p',$$
$$n''=2q,$$
$$n'''=r.$$

If $t=2$, $n'''=\frac{2r}{m}$. In this case m may have one of two values, $m=1$ and $m=2$. In the case $m=1$, we have

$$n=p+p',$$
$$n'=2p',$$
$$n''=2q,$$
$$n'''=2r.$$

In the case $m=2$,

$$n=\frac{p+p'}{2},$$
$$n'=p',$$
$$n''=q,$$
$$n'''=r.$$

II. m is of the form $2m_1$, where m_1 is a factor of p' and q. In this case we have

$$n=\frac{p+p'}{2m_1},$$
$$n'=\frac{p'}{m_1},$$
$$n''=\frac{q}{m_1},$$
$$n'''=\frac{tr}{2m_1}.$$

(1) If $t=1$, $n'''=\frac{r}{2m_1}$, $m_1=1$, and

$$n=\frac{p+p'}{2},$$
$$n'=p',$$
$$n''=q,$$
$$n'''=\frac{r}{2}.$$

(2) If $t=2$, $n'''=\frac{r}{m_1}$, $m_1=1$, and

$$n=\frac{p+p'}{2},$$
$$n'=p',$$
$$n''=q,$$
$$n'''=r.$$

It appears therefore that, in the case of the hypothesis that the symbol of hydrogen is expressed by one prime factor α, m (as in the case of the previous hypothesis) may have two values and two values only, $m=1$, $m=2$. If $m=1$ no restriction is placed on the values of the integers p, p', q, r; but if $m=2$, $p+p'$ must be an even number.

Referring again to the system of equations

$$nm=p+p',$$
$$n'm=2p',$$
$$n''m=2q,$$
$$n'''m=tr,$$

we have from these equations

$$\frac{n'}{n}=\frac{2p'}{p+p'}.$$

Now the values of the fraction $\frac{2p'}{p+p'}$ for all positive and integral values of p and p' (whatever be the value of m) are comprehended between the limits 0 and 2.

If $p'=0$,

$$\frac{2p'}{p+p'}=0.$$

If $p=0$,

$$\frac{2p'}{p+p'}=2.$$

It hence appears that if the symbol $\alpha^{n}\chi^{n'}\xi^{n''}x^{n'''}$ is to satisfy the conditions given in the equation

$$m\alpha^{n}\chi^{n'}\xi^{n''}x^{n'''}=p\alpha+p'\alpha\chi^{2}+q\xi^{2}+rx',$$

a restriction is placed by the conditions of the problem upon the values of the integers n and n', which must be so selected that $\frac{n'}{n}$ shall not be <0 or >2.

If $\frac{n'}{n}=0$, $p'=0$, and the equation becomes

$$m\alpha^{n}\xi^{n''}x^{n'''}=p\alpha+q\xi^{2}+rx'.$$

If $\frac{n'}{n}=2$, $p=0$, and the equation becomes

$$m\alpha^{n}\chi^{2n}\xi^{n''}x^{n'''}=p'\alpha\chi^{2}+q\xi^{2}+rx'.$$

No restriction exists on the values of n'' and n'''. We may notice the two forms :—

$$(1)\qquad \begin{aligned} m&=1,\\ n&=p+p',\\ n'&=2p',\\ n''&=2q,\\ &\left\{\begin{aligned} n'''&=r,\\ n'''&=2r.\end{aligned}\right.\end{aligned}$$

$$(2)\qquad \begin{aligned} m&=2,\\ n&=\frac{p+p'}{2},\\ n'&=p',\\ n''&=q,\\ &\left\{\begin{aligned} n'''&=r,\\ n'''&=\frac{r}{2}.\end{aligned}\right.\end{aligned}$$

For the interpretation of the symbol of a unit of matter it is essential that we should be informed of the conditions which that unit is to satisfy, apart from which no interpretation is possible, for the same symbol may have its origin in many ways. The way in which the symbol originates is defined by the equations; thus, in the present case, to interpret the symbols $\alpha^{n}\chi^{n'}\xi^{n''}\kappa^{n'''}$,

> n is the ratio of the sum of the units of hydrogen and chlorine formed to the number of units of matter decomposed;
>
> n' is twice the ratio of the number of units of chlorine formed to the number of units of matter decomposed;
>
> n'' is twice the ratio of the number of units of oxygen formed to the number of units of matter decomposed;
>
> n''' is t times the ratio of the number of units of carbon formed to the number of units of matter decomposed, t being (probably) 1 or 2.

For example, to interpret the symbols $\alpha\chi$, $\alpha\xi$. In both cases $n=1$. That is to say, $\alpha\chi$ and $\alpha\xi$ are symbols of two units of matter such that the sum of the units of hydrogen and chlorine formed by their respective decompositions is equal to the number of units thus decomposed. In the case of $\alpha\chi$, $n'=1$, $n''=0$, $n'''=0$, that is to say, the ratio of the units of chlorine formed to the units of matter decomposed is as $1:2$. No oxygen and no carbon are formed in the decomposition. In the case of $\alpha\xi$, $n'=0$, $n''=1$, $n'''=0$. The ratio of the units of oxygen formed to the units of matter decomposed is as $1:2$, no chlorine and no carbon are formed. These properties characterize the respective units of hydrochloric acid and of water. Again, in the case of the symbol $\alpha^{2}\chi\xi\kappa$ (the symbol of the chloride of acetyl), $n=2$. The sum of the units of hydrogen and chlorine formed by the decomposition of the substance is twice the number of units of the substance decomposed. $n'=1$, the ratio of the units of chlorine found to the units of matter decomposed is as $1:2$. $n''=1$, the ratio of the units of oxygen found to the units of matter decomposed is also $1:2$. $n'''=1$, the ratio of the units of carbon formed to the units of matter decomposed is as $1:t$, where t is assumed to be 1 or 2. We hence are always able, from inspection of the symbol, to reconstitute the equation whence that symbol was derived.

The preceding reasoning may be extended to the general case. Let m units of any chemical substance $\alpha^{n}\chi^{n'}\omega^{n'_1}\nu^{n'_2}\ldots\xi^{n''}\theta^{n''_1}\lambda^{n''_2}\ldots\kappa^{n'''}\delta^{n'''_1}\ldots$ be identical with p units of hydrogen α, p' units of chlorine $\alpha\chi^{2}$, p'' units of iodine $\alpha\omega^{2}$, p''' units of nitrogen $\alpha\nu^{2}\ldots$, q units of oxygen ξ^{2}, q' units of sulphur θ^{2}, q'' units of selenium $\lambda^{2}\ldots$, r units of carbon κ, r' units of mercury δ, r'' units of zinc $\zeta\ldots$, so that

$$m\alpha^{n}\chi^{n'}\omega^{n'_1}\nu^{n'_2}\ldots\xi^{n''}\theta^{n''_1}\lambda^{n''_2}\ldots\kappa^{n'''}\delta^{n'''_1}\ldots=p\alpha+p'\alpha\chi^{2}+p''\alpha\omega^{2}+p'''\alpha\nu^{2}+\ldots$$
$$+q\xi^{2}+q'\theta^{2}+q''\lambda^{2}+\ldots+r\kappa+r'\delta+r''\zeta+\ldots,$$

we have then the following equations connecting the positive integers $m, n, n', \ldots$ $p, p', p'', \ldots$

$$mn=p+p'+p''+p'''+\dots$$

$$mn' =2p' \,,\ n' = \frac{2p'}{m},$$

$$mn'_1 =2p'' \,,\ n'_1 = \frac{2p''}{m},$$

$$mn'_2 =2p''' ,\ n'_2 = \frac{2p'''}{m},$$

$$\dots\dots\quad\dots$$

$$\dots\dots\quad\dots$$

$$mn'' =2q \,,\ n'' = \frac{2q}{m},$$

$$mn''_1 =2q' \,,\ n''_1 = \frac{2q'}{m},$$

$$mn''_2 =2q'' ,\ n''_2 = \frac{2q''}{m},$$

$$\dots\dots\quad\dots$$

$$\dots\dots\quad\dots$$

$$mn'''=r \quad,\ n'''=\frac{r}{m},$$

$$mn'''_1=r' \quad,\ n'''_1=\frac{r'}{m},$$

$$mn'''_2=r'' \quad,\ n'''_2=\frac{r''}{m},$$

$$\dots\dots\quad\dots$$

$$\dots\dots\quad\dots;$$

whence, putting

$$\begin{aligned}
\Sigma(p') &=p' +p'' +p'''+ \dots,\\
\Sigma(q) &=q +q' +q'' + \dots,\\
\Sigma(r) &=r +r' +r'' + \dots,\\
\Sigma(n') &=n' +n'_1 +n'_2 + \dots,\\
\Sigma(n'') &=n''+n''_1 +n''_2+ \dots,\\
\Sigma(n''') &=n'''+n'''_1 +n'''_2+ \dots,
\end{aligned}$$

we have

$$n=\frac{p+\Sigma(p')}{m},$$

$$\Sigma(n')=\frac{2\Sigma(p')}{m},$$

$$\Sigma(n'')=\frac{2\Sigma(q)}{m},$$

$$\Sigma(n''')=\frac{\Sigma(r)}{m}.$$

And reasoning as before, since m is prime to some one, at least, among the numbers $p, p', p'', p''', \ldots q, q', q'', q''' \ldots r, r', r'' \ldots$, if m be prime to some one among the numbers $r, r', r'', \ldots m=1$. If m be prime to any one among the numbers $p', p'', p''', \ldots q, q', q''$, and be not prime to any one among the numbers $r, r', r'' \ldots, m=2$. If m be not prime to any one among the numbers $p', p'', p''', \ldots q, q', q'', \ldots r, r', r'', \ldots m$ is also not prime to p, which is contrary to the construction. Therefore $m=1$ or $m=2$.

$$\text{If } m=1, \quad n=p+\Sigma(p'),$$

$$\Sigma(n')=2\Sigma(p');$$

$$\text{if } m=2, \quad n=\frac{p+\Sigma(p')}{2},$$

$$\Sigma(n')=\Sigma(p'),$$

the condition being that $p+\Sigma(p')$ is to be an even number.

We have also

$$\frac{\Sigma(n')}{n}=\frac{2\Sigma(p')}{p+\Sigma(p')};$$

whence, in addition to the conditions

$$\frac{n'}{n} \text{ not } <0 \text{ not } >2,$$

$$\frac{n'_1}{n} \quad \text{,,} \quad \text{,,} \quad \text{,,}$$

$$\frac{n'_2}{n} \quad \text{,,} \quad \text{,,} \quad \text{,,}$$

we have

$$\frac{\Sigma(n')}{n} \text{ not } >2.$$

The symbol $\alpha^{n}\chi^{n'}\omega^{n'_1}\ldots\xi^{n''}\theta^{n''_1}\ldots\kappa^{n'''}\delta^{n'''_1}\ldots$ is to be interpreted on the same principles as the symbol $\alpha^{n}\chi^{n'}\xi^{n''}\kappa^{n'''}$ previously considered. Thus to interpret the symbol $\alpha\chi\omega$, here $n=1$, the sum of the units of hydrogen, chlorine, and iodine formed by the decomposition of the substance when decomposed into these elements is equal to the number of units of the substance thus decomposed, $n'=1$, $n'_1=1$; of the units of hydrogen, chlorine, iodine thus formed $\frac{1}{2}$ are units of iodine and $\frac{1}{2}$ units of chlorine, no hydrogen being formed. These are the characteristic properties of the unit of chloride of iodine.

This reasoning may be further extended so as to include the case of the elements phosphorus $\alpha^2\varphi^4$ and other elements similar to it in form.

Both hypotheses, therefore, so far agree that each covers the whole ground, and includes all known compounds of carbon, hydrogen, oxygen, chlorine ... But the system of combinations constructed upon hypothesis α does not include those pretended compounds of carbon, hydrogen, oxygen, chlorine (recognized as possible compounds of these elements in the other system) in the case of which the sum of the units of hydrogen, chlorine . . . (and generally the sum of the units of the dyad elements) formed by the decomposition of two units of the compound is other than an even number. Hypothesis α affords no means of manufacturing these compounds; so that we are led to the conclusion that such things are to be regarded as mere "non-entities" or "monstrosities," combinations of incompatible ingredients, which cannot be made up, by any known chemical operations, of the matter of those actual chemical elements out of which all known chemical existences are constructed[*]. It is hardly necessary to observe that such things cannot, as a matter of fact, be made—the limitation introduced by hypothesis α being the celebrated law of Even Numbers, discovered as an empirical truth by LAURENT and GERHARDT, which is here referred to its origin, and deductively established, as a consequence of this hypothesis.

Were we to proceed to enumerate the combinations of hydrogen, chlorine, nitrogen, and the other dyad elements, as indicated by the two hypotheses respectively, and compare the systems thus indicated with the systems actually existing, we should find that the combinations indicated in the case of hypothesis α^2 were twice as numerous as in the case of hypothesis α; and we should, in fact, on this hypothesis (α^2) make a blunder in every alternate statement, knowing neither more nor less about the result than we do in tossing a halfpenny whether it will fall head or tail—a position very different to that of one who, having obtained information that the coin is weighted, never makes any such mistakes at all, which is our position on hypothesis α. That the conclusions to which we are thus brought as to the nature and relations of these dyad elements are truly of a remarkable character is not to be denied; but it is a sufficient reply to those persons who make difficulties on this score, that they may just as reasonably object to the facts on which these conclusions are based, which are equally remarkable with the conclusions; and, moreover, that the value of an hypothesis is to be tested, not by the coincidence of its conclusions with our previous expectations, but by their agreement with the facts which we wish to explain.

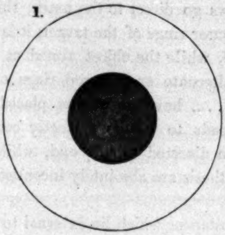
Now it may be contended, admitting it to be true that hypothesis α puts a correct limitation upon these combinations, yet this advantage is gained at too great a cost. It rejects indeed (it may be said), and rightly, the combinations rejected by the empirical law of Even Numbers, but, in so doing, opens the door of the system to a large number of compounds totally unknown to us. Thus while we eradicate one set of impossible existences, we introduce another. Where, it is asked, are the elements $\chi, \omega, \nu \ldots$, or such combinations of these elements as the units $\alpha\chi^3$, $\chi\xi$, $\chi\nu$, $\kappa\chi^2$ . . . and the like? But this argument does not come to much. Our only choice lies between the two hypotheses in question, and hypothesis α^2 is attended with far graver difficulties; for in this system the real and unreal combinations are mixed up together, which is not the case on hypothesis α. On this latter assumption we do not introduce among the units made up of the matter of our actual elemental bodies the unit of any chemical substance whatever which cannot be thus made up; so that the system with which we have to work makes no false assertion, but is from the beginning properly constructed. A sharp line of demarcation is drawn in it between what is and what is not, our actual system of chemical combinations appearing to us as a fragment of a wider and unrealized system, of which it is a part and in which it is comprehended.

---

[*] "Desinit in piscem mulier formosa superne." The construction of the unit of a chemical substance to be resolved into two units of hydrogen and three units of chlorine presents the same order of difficulties as the combination of the head of a woman with the tail of a fish. We cannot imagine how such a thing could be produced!

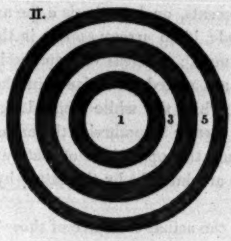
The relation of the two hypotheses will be readily appreciated from the following diagrams:—

I.



Hypothesis α.

II.



Hypothesis α^2.

Figure I. represents to us the system of combinations on hypothesis α. The total area included within the outer contour indicates what I may term the region of possible combinations—that is to say, of the combinations, actual or conceivable, of the simple weights of the system α, χ, ξ, ω, ν . . . by which we may consider it to be occupied. This region is divided into two subordinate regions—the region of actual existences, namely, the region occupied by the combinations made up of the matter of our actual elemental bodies, hydrogen, chlorine, iodine, nitrogen, oxygen, carbon, and the like, indicated by the shaded space; and an unknown or unexplored region, external to this, containing the combinations of these same simple weights which are not thus made up and of which we have no actual representatives. Were we to write down in each region the symbols of the combinations by which we thus consider it to be inhabited, we should place within the shaded space the symbols satisfying the conditions given by the law of even numbers, and in the open space the symbols which do not satisfy those conditions.

Figure II. is constructed on the same principles, to indicate the combinations on hypothesis a^2. The area within the outer line comprehends the region of possible combinations, actual or conceivable, of the simple weights of the system* $a, c, i, n, \xi, \ldots$, all of which are constituted of the matter of our actual elemental bodies, and are capable of being resolved into them. But this region is divided into a system of annular spaces, of which only each alternate ring is actually inhabited. The occupied regions are indicated by the shaded spaces, and are assumed to be tenanted by those substances which satisfy the law of even numbers; while the unexplored regions, indicated by the open rings, are occupied by the imaginary substances which do not satisfy this law. It is therefore a mere mistake to consider hypothesis α to be in any sense more hypothetical than hypothesis a^2. Both hypotheses indicate to us the possible existence of a system of unreal things; but there is a wide difference in the relation subsisting between the realities and unrealities on the two views respectively. If we consider the operations of nature to be directed, as a flight of arrows at a target, to the construction out of the matter of our elemental bodies of a system of chemical substances made up of the matter of those elements, on hypothesis α the arrows go direct to the mark, this aim is properly attained; if no arrows appear in the outer rings of the target, it is that they lie wide of the mark; whereas on hypothesis a^2, while the object aimed at is the same, the arrows are placed exclusively in the alternate or coloured rings of the target, 2, 4, 6, . . ., while the white rings, 1, 3, 5, . . ., have no arrows placed in them at all†. The former hypothesis, therefore, leads to results perfectly consistent with the view that the operations of nature are directed to this end, while the results to which we are brought by the latter hypothesis are absolutely inconsistent with this view.

Although the actual existence of those substances which lie external to our system is by no means a necessary consequence of our hypothesis (for we certainly cannot expect to be able to do all that is possible to be done), and the construction of these substances may be a problem altogether beyond the range of our experimental powers, yet it must be admitted by any who hold to the principles of this method that the appearance upon the scene of these "ideal existences" is at least a possible phenomenon; and it is not without interest to consider our position in regard to this contingency. Two reasons may be given for believing this to be possible: the one, of an abstract kind, derived from the fundamental property of chemical symbols given in the equation

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* *i. e.* $\frac{H}{2}, \frac{Cl}{2}, \frac{I}{2}, \frac{N}{2}, \frac{O}{2}, \ldots$

† In figure I. we should have within the central shaded space in the case of the combinations $\alpha^{n}\chi^{n_1}\omega^{n_2}\ldots$, those combinations in which the ratios $\frac{n_1}{n}, \frac{n_2}{n}, \ldots$ were severally not <0 not >2, while in the open space the combinations would appear in which this condition was not satisfied. In figure II., considering the symbol $a^{m}c^{m_1}i^{m_2}, \ldots$, we should have in the shaded spaces 2, 4, 6, . . ., the combinations of these letters taken 2 and 2, 4 and 4, 6 and 6, . . . together, which combinations are represented in our actual system of things; while in the open spaces, 1, 3, 5, . . . we should have the combinations of these same simple weights taken singly, 3 and 3, 5 and 5, . . ., which are non-existent combinations.

$$\alpha\chi^2=\alpha+2\chi,$$

whence it may be argued that, as we can theoretically resolve $\alpha\chi^2$ into α and χ and χ, we are not justified in shutting our eyes to the possibility that the unit of chlorine may actually be resolved into a unit of hydrogen and two units of the (as yet) unknown element χ. The argument is just, but the presumption thus raised is excessively slight; for we are able to make (on any hypothesis) so very small a proportion of the vast number of chemical substances, the possible existence of which is similarly indicated to us, that we have no expectation, capable of being estimated, of making any one substance in particular. In short, the thing is possible; but on these grounds alone we cannot expect to be able to do it. The second argument is of far greater weight. It may be put thus:—All persons would admit the validity of our conclusions if, having first detected the element χ, thus latent in its compounds, we were able to verify our hypothesis by digging it out. χ having been discovered, there would be but little doubt as to the existence of ω and ν. We have not got quite to this point, but nevertheless we are not so very far from it; for we can show clearly that the application of the very same formal principles of reasoning to existing facts would have enabled us similarly to detect, prior to their isolation, the latent existence in their compounds of elements which have actually been isolated[*].

Were we able to conduct our chemical experiments only between certain fixed limits of temperature (let us say between 0° and 300° C.), a barrier would be placed upon our researches. We might still conduct innumerable experiments, but the science which resulted from them would be essentially different to our present chemistry. Between these limits of temperature the appearance of the element carbon in any system of chemical transformations is a rare phenomenon indeed; and it is by no means difficult to place ourselves, by an effort of imagination, in the position of chemists who should have a very varied and extensive knowledge of the chemical properties of the compounds of carbon, and yet be totally unaware of the existence of that element. Let us imagine, then, some unknown world, some Laputa (devoted to philosophy) in which the chemistry was of this order. How would the Laputians proceed with the scientific construction of the symbols of the units of matter? Two units of marsh-gas (they would say) are identical with three units of hydrogen and a unit of acetylene. Having solved a similar problem in the case of ammonia [I. Sec. VII. (8)], they have no difficulty in at once writing down as follows the symbols of these units on the two hypotheses α and α^2 respectively:—

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[*] Chemists are placed in regard to the ideal elements χ, ω, β, ν, and those combinations of them which cannot be resolved into our actual elemental bodies, in a very similar position to that occupied by astronomers (if I may venture on such a comparison) towards the planet Neptune prior to its detection by the telescope. The existence of the planet Neptune could be inferred, as a highly probable contingency, from the perturbations of the orbit of Uranus; and yet this planet might have remained for ever unseen by man. So, too, the existence of the element χ may be similarly inferred from the peculiar forms of chemical metamorphosis; and yet this element may never be isolated as an independent reality. It may exist and exercise (so to say) an appreciable influence upon the movements of our system, and nevertheless lie far beyond the range of facts accessible to our methods of experiment. No scientific hypothesis can have any claim whatever upon our notice which is incapable of experimental verification (a truth too often forgotten in chemical speculations); but this verification may be a very tardy process, and involves numerous trials and numerous failures before in either sense (so as to enable us to deny or affirm the hypothesis) it is accomplished.

	Hypothesis α.	Hypothesis α^2.	Weight in grm.
Hydrogen	α	α^2	0·089
Marsh-gas	$\alpha^2 x$	$\alpha^3 \upsilon$	0·704
Acetylene	αx^2	υ^2	1·161

The simple weight υ, which appears in the symbols as expressed on hypothesis α^2, weighs 0·58 grm.

We shall suppose our chemists to be in possession of those substances of which the matter is identical with the matter of acetylene, hydrogen, oxygen, chlorine, and our other elements, the material of an ample chemistry. Proceeding with the construction of the symbols of the units of matter on the two hypotheses, they would find both to be applicable as indicated in the following examples:—

	Hypothesis α.	Hypothesis α^2.
Benzol	$\alpha^3 x^6$	υ^6
Olefiant gas	$\alpha^2 x^2$	$\alpha^2 \upsilon^2$
Allylene	$\alpha^2 x^3$	$\alpha \upsilon^3$
Methyl	$\alpha^3 x^2$	$\alpha^4 \upsilon^2$
Propylene	$\alpha^3 x^3$	$\alpha^3 \upsilon^3$
Ethyl	$\alpha^5 x^4$	$\alpha^6 \upsilon^4$
Allyl	$\alpha^5 x^6$	$\alpha^4 \upsilon^6$
Formic acid	$\alpha x \xi^2$	$\alpha \upsilon \xi^2$
Methylic alcohol	$\alpha^2 x \xi$	$\alpha^3 \upsilon \xi$
Oxide of ethylene	$\alpha^2 x^2 \xi$	$\alpha^2 \upsilon^2 \xi$
Alcohol	$\alpha^3 x^2 \xi$	$\alpha^4 \upsilon^2 \xi$
Benzylic alcohol	$\alpha^4 x^7 \xi$	$\alpha \upsilon^7 \xi$
Acetic acid	$\alpha^2 x^2 \xi^2$	$\alpha^2 \upsilon^2 \xi^2$
Glycol	$\alpha^3 x^2 \xi^2$	$\alpha^4 \upsilon^2 \xi^2$
Glycerine	$\alpha^4 x^3 \xi^3$	$\alpha^5 \upsilon^3 \xi^3$
Anhydrous acetic acid . . .	$\alpha^3 x^4 \xi^3$	$\alpha^2 \upsilon^4 \xi^3$
Acetic peroxide	$\alpha^3 x^4 \xi^4$	$\alpha^2 \upsilon^4 \xi^4$
Lactic acid	$\alpha^3 x^3 \xi^3$	$\alpha^3 \upsilon^3 \xi^3$
Chloride of methyl	$\alpha^2 x \chi$	$\alpha^2 \upsilon c$
,, ,, ,, ...	$\alpha^2 x \chi^2$	$\alpha \upsilon c^2$
Chloroform	$\alpha^2 x \chi^3$	υc^3

Chloride of ethylene	$\alpha^2\kappa^2\chi^2$	$\alpha\upsilon^2c^2$
Chloride of acetyl	$\alpha^2\chi\kappa^2\xi$	$\alpha\upsilon^2c\xi$
Chloracetic acid	$\alpha^2\chi\kappa^2\xi^2$	$\alpha\upsilon^2c\xi^2$
Chloriodide of ethylene	$\alpha^3\chi\omega\kappa^2$	$\alpha^2\upsilon^2ci$
Hydrocyanic acid	$\alpha\nu\kappa$	υn
Methylamine	$\alpha^3\nu\kappa$	$\alpha^2\upsilon n$
Trimethylamine	$\alpha^5\nu\kappa^3$	$\alpha^3\upsilon^3n$

It is thus evident that the Laputian philosophers might have proceeded a long way indeed in the study of the metamorphoses of the compounds of carbon, and yet take a very different view of the nature of these compounds to that taken by ourselves. In their eyes the group of hydrocarbons would appear as compounds of hydrogen and the element acetylene, of which they would possess in benzol an allotropic modification. Besides these they might have innumerable compounds of acetylene and their other elements, of the kind indicated in the Table.

Proceeding with their deductive investigations, they would arrive at the law of even numbers, and applying this critical test to the compounds of acetylene, would arrive at the following equations:—

$$2\alpha^2\kappa=3\alpha+\alpha\kappa^2,$$
$$2\alpha^2\kappa^3=\alpha+3\alpha\kappa^2,$$
$$2\alpha^3\kappa^3=3\alpha+3\alpha\kappa^2,$$
$$\alpha^3\kappa^4=\alpha+2\alpha\kappa^2,$$
$$2\alpha^2\kappa\chi^2=2\alpha+\alpha\kappa^2+\alpha\chi^2,$$
$$2\alpha^2\kappa\chi^3=3\alpha\chi^2+\alpha\kappa^2,$$
$$2\alpha^2\kappa\chi^2\xi=\alpha+2\alpha\chi^2+\alpha\kappa^2+\xi^2,$$
$$2\alpha^2\chi\kappa^2\xi=\alpha+\alpha\chi^2+2\alpha\kappa^2+2\xi^2,$$
$$2\alpha^3\chi\omega\kappa^2=2\alpha+\alpha\chi^2+\alpha\omega^2+2\alpha\kappa^2,$$

. . . .

. . . .

The Laputian philosophers may be assumed to have had great reliance on their deductive processes, and would doubtless have said at once that these facts afforded a solid ground of preference in favour of their hypothesis α—indeed that the other hypothesis was relatively untenable. Nevertheless we may believe them to have felt some satisfaction when, on descending from their floating island to our earth, we showed them the process by which acetylene was manufactured by BERTHELOT from hydrogen and carbon, and thus turned their hypothesis into a demonstrated truth.

There is, however, a difficulty in the way of the unqualified acceptance of hypothesis α, of a very real character, namely that it does not, *primâ facie*, appear to be in perfect concordance with facts. Now the facts to which this remark applies are of two classes:—(1) The assumed densities of gases which are not in accordance with either of the two hypotheses α and a^2, and (2) assumed densities which are not *primâ facie* in accordance with hypothesis α, but which are not discordant with hypothesis α^2. In the former case the units of matter cannot be expressed by an integral number of prime factors, either on hypothesis α or on hypothesis α^2; in the second case the units of matter can be expressed by an integral number of prime factors on hypothesis α^2, but cannot be so expressed on hypothesis α. The question has been (up to a certain point) discussed in Part I. Section VIII., where a Table is given of these exceptions [Part I. Section VIII. (3)].

I shall not enter on the former class of exceptions, not only for the reason that the difficulties presented by them are not peculiar to this method, but also that these obstacles are rapidly being removed by a careful scrutiny of the facts, and so many such obstacles have actually disappeared that the removal of the rest must be regarded simply as a question of time and trouble. The second class of exceptions, however, stands on a very different footing. They go to the very root of the matter, and no rational explanation of these exceptions has (so far as I am aware) been ever suggested from the present point of view of chemistry. We will fix our attention on two salient examples, the binoxide and tetroxide of nitrogen.

Assuming the gaseous densities of these substances as given by various observers to be the true densities of homogeneous gases, it must be admitted that the ponderable matter of two units of binoxide of nitrogen is identical with the ponderable matter of a unit of nitrogen and a unit of oxygen; and also it must be admitted that the ponderable matter of two units of tetroxide of nitrogen is identical with the ponderable matter of a unit of nitrogen and two units of oxygen. The facts here stated are incompatible with the expression of the symbols of the units of the binoxide of nitrogen and of the tetroxide of nitrogen respectively by an integral number of prime factors, on the hypothesis that the symbol of hydrogen is expressed as α [Part I. Section VIII. (2)]; for we cannot find, on that hypothesis, any positive and integral solutions of the indeterminate equations connecting the integers, which are the indices of the prime factors by which these symbols are expressed.

Now if this be regarded as a true statement of the facts, it must be allowed that the binoxide and tetroxide of nitrogen are not only (what indeed, perhaps without exception, they are) the most curious of all chemical substances[*], but are absolutely unique objects not made in the same way as other things, or turned out of the same workshop with them; in short (as I before said) they must be "chemical monstrosities," "lusus naturæ," which cannot be generated by any known process of generation. Without venturing to impose any limit on the powers of nature, we yet should think twice about the evidence before committing ourselves to belief in a mermaid; and I must confess that I feel a similar hesitation in believing in the existence of any chemical substance made out of hydrogen, oxygen, and nitrogen which cannot be made by the operations α, ξ, and ν. We are not, however, driven into this corner.

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[*] LAURENT endeavours to get out of the difficulty by drawing a distinction between the molecule and (what I have termed) the unit. The unit, he says, is indeed NO, but the molecule is N_2O_2. As, however, a molecule can only be defined by referring it to the unit and saying that in all cases two units of matter are constituted of an equal number of molecules, this explanation is not very satisfactory, although it is a path in which LAURENT has been contentedly followed by the greater number of chemists. LAURENT was fully alive to the difficulties of the case, as appears from the following sentence:—"Unfortunately nitric oxide and peroxide of nitrogen are such singular bodies that it appears somewhat difficult to discover their analogues," taken together with the note attached ('Chemical Method,' translation, Cavendish Society, 1855, p. 82).

First, we may notice in regard to one of these two substances, namely, hyponitric acid, that the observed density of the gas is not a constant, but varies with the temperature at which the observation is made. This point has been observed in three independent sets of experiments by PLAYFAIR and WANKLYN, by MÜLLER and by DEVILLE, the results being in the main concordant. When the density is taken at a low temperature, it approximates to, although it does not reach, the number corresponding to the symbol $\alpha\nu^2\xi^4$; thus we have:—

Observer.	Temperature.	Density (Air 1).	Density (Hyd. 1).
DEVILLE	26°·7	2·65	38·24
MÜLLER	20	2·70	38·96
PLAYFAIR and WANKLYN . .	24·5	2·52	36·39
„ „ . .	11·3	2·64	38·19
„ „ . .	4·2	2·59	

The density corresponding to the symbol $\alpha\nu^2\xi^4$ is 46·0, showing a difference between this number and the lowest observed density of about 15 per cent.

As the temperature of the gas increased the density diminished, until the observed density (in the observation made) attained a numerical value of 1·57, after which the observations ceased.

In the following Table these densities are calculated on the hydrogen scale, and also in the usual manner, the density of air being assumed as 1:—

Temperature.	Density (Air 1).	Density (Hydrogen 1).
100°·1	1·68	24·24
111·3	1·65	23·80
121·5	1·62	23·35
135·0	1·60	23·14
154·0	1·58	22·80
183·2	1·57	22·60

Now the theoretical density of the tetroxide of nitrogen symbolized as $\alpha\nu^2\xi^4$ is on the hydrogen-scale 46, and on the air-scale 3·19. Half this density, therefore, is on the hydrogen-scale 23, and on the air-scale 1·59. The conclusion to be drawn from these numbers, therefore, is that the gas undergoes a gradual expansion with the increase of temperature until at about 150° this limit is attained, so that the constitution of the gas at the lower temperature is essentially different from its constitution at the higher temperature. But in what does this difference consist?

If we are to discuss such questions with advantage, we must be content to advance from the known to the unknown, and base our reasoning upon analogous cases with which we are already acquainted. Here, again, I shall venture to give an illustration of the subject from the chemistry of Laputa.

The Laputians, like ourselves, had their chemical difficulties. Two problems especially had perplexed the heads of their philosophers (although, indeed, some of their chemists saw no difficulty in them at all). They had in their possession two chemical substances, to the units of which they had for various reasons assigned the symbols $\alpha\kappa^2\xi^2$ and $\alpha\kappa^2\xi^4$, and of which they had confidently predicted the densities (or weights of the units) on their hydrogen standard to be respectively 29 and 45. When, however, their best experimentalists came to take the density of these gases, this density was found to be in each case only half that at which it had been estimated, namely 14·5 in the case of $\alpha\kappa^2\xi^2$, and 22·5 in the case of $\alpha\kappa^2\xi^4$.

The difficulty was serious enough; for hitherto in the construction of the units of matter from acetylene $\alpha\kappa^2$, oxygen ξ^2, hydrogen α, and the other elements, the operations α, κ, ξ . . . had been sufficient for every requirement, hundreds of things had been made with these tools. But if these statements were to be unreservedly accepted, these operations would be inadequate to the purposes of the science; for the symbols of the units of these substances, as expressed by the letters α, κ, ξ, would be $\alpha^{\frac{1}{2}}\kappa\xi$, $\alpha^{\frac{1}{2}}\kappa\xi^2$, necessitating in their construction the performance of an operation $\alpha^{\frac{1}{2}}$, of which they had no experience whatever.

The unit $\alpha\kappa^2\xi^2$ is the unit of a substance which the Laputians had, but which we have not. It goes in our language under the name of the radical of formic acid. $\alpha\kappa^2\xi^4$ is the symbol we assign to the unit of oxalic acid. I proceed to explain how the Laputians dealt with this question.

"The accuracy of our experimentalists," said they, "is beyond suspicion, and we accept all the statements of fact made by them. But there is one point as to which it is necessary to inquire. How do they know, as their objections imply, that the gases of which they have taken the densities are really homogeneous gases? If at the temperature at which the experiment has been made these gases are really split in two, it is not necessary to modify, in this sense, our views.

"Now similar impediments have frequently stood in the way of our theories. But wherever it has been possible to look into the matter, as in the case of the pentachloride of phosphorus, and even hydrated sulphuric acid, theory has come out triumphant. This case is not strictly similar to those previously dealt with, but yet is not so dissimilar as to lead us to look for a totally different account of the matter. That we should discover a new tool $\alpha^{\frac{1}{2}}$ does not, we must confess, appear to us very likely. But that we should be able to do more, with our old tools α, κ, ξ, than we have hitherto done is by no means out of the question. For the sake of argument let us assume, then, that the unit $\alpha\kappa^2\xi^2$, our binoxide of acetylene, cannot exist as a gas, but is invariably resolved into the two units $\alpha\kappa\xi$ and $\kappa\xi$ according to the equation

$$\alpha\kappa^2\xi^2=(\alpha\kappa\xi+\kappa\xi).$$

"The weight of a unit of these mixed gases will be precisely that given by your experiments, namely 14·5; and, moreover, it appears to us that we can explain all that we, or you, know about the gas from this point of view.

"The two units jointly (and you know no more) are identical with a unit of acetylene and a unit of oxygen; thus

$$(\alpha\kappa\xi+\kappa\xi)=\alpha\kappa^2+\xi^2.$$

"Five units of hydrogen and two units of your gas are identical with and capable of being resolved into two units of $\alpha^2\kappa$ (marsh-gas) and two units of $\alpha\xi$ (water); thus

$$5\alpha+(\alpha\kappa\xi+\kappa\xi)=2\alpha^2\kappa+2\alpha\xi,$$

which equation is identical with the sum of the two equations

$$2\alpha+\alpha\kappa\xi=\alpha^2\kappa+\alpha\xi,$$

$$3\alpha+\kappa\xi=\alpha^2\kappa+\alpha\xi.$$

"If you ask us to explain the origin of the unit $\alpha\kappa^2\xi^4$ (oxalic acid) by the oxidation of the unit $\alpha\kappa^2\xi^2$, we may go further in the same direction. We have

$$(\alpha\kappa\xi+\kappa\xi)+\xi^2=(\alpha\kappa\xi^2+\kappa\xi^2).$$

"Now the weight of a unit of the mixed gases $\alpha\kappa\xi^2$ (formic acid) and $\kappa\xi^2$ is precisely that which you have assigned to the unit $\alpha\kappa^2\xi^4$, namely 22·5.

"It is easy to render an account, on this assumption, of the chemical properties of this unit.

"Thus a unit of $\alpha\kappa^2\xi^4$ (oxalic acid), together with a unit of water, is identical with the unit $\alpha\kappa\xi^2$ (formic acid) and the unit $\alpha\kappa\xi^3$ (hydrated carbonic acid); thus

$$(\alpha\kappa\xi^2+\kappa\xi^2)+\alpha\xi=\alpha\kappa\xi^2+\alpha\kappa\xi^3.$$

"Again, three units of $\alpha\kappa^2\xi^4$, together with two units of water, are identical with one unit $\alpha\kappa^2\xi^2$ and four units $\alpha\kappa\xi^3$; thus

$$3(\alpha\kappa\xi^2+\kappa\xi^2)+2\alpha\xi=(\alpha\kappa\xi+\kappa\xi)+4\alpha\kappa\xi^3."$$

When the difficulty was pressed home upon the Laputian philosophers, that they were mere dreamers, for that their chemists had worked with these gases for more than 100 years and had never detected the presence in them of any one of these imaginary existences $\kappa\xi$, $\kappa\xi^2$, $\alpha\kappa\xi$, $\alpha\kappa\xi^2$, they replied that while it was true that many interesting objects lay upon the surface, so that anybody could see them, there were others not to be found unless they were specially looked for, and that no chemist had ever yet looked for these things. Also the difficulties in the way of their separation might be very great. The densities of these gases would be so nearly the same that it would be hopeless to attempt to separate them by diffusion. As to the similarity, up to a certain point, of the chemical properties of these substances $\alpha\kappa\xi$ and $\kappa\xi$ which it was necessary to assume, this need be no matter of surprise at all, for chemical substances (as everybody knew) existed in groups characterized by such similarities. When it was further urged that it would be very odd for such things to be found there and nowhere else in the world, they replied that on this point they really were incompetent to judge, but that, at any rate, many things as strange had more than once happened before, as in the case of the elements cæsium, rubidium, and lithium, three out-of-the-way things found all together in one mineral water, and the mineral gadolinite, which presented many curious problems which no one had yet fathomed. However, they added, "We always measure out our belief according to the evidence, and you are mistaken if you imagine us to be speaking over-confidently on such a subject."

Now we certainly cannot say that any Laputian chemists were placed in the position or reasoned in the way we have described; but we may say that if they had been placed in this position and reasoned thus they would have reasoned correctly, and their reasoning would have been fully justified by facts.

This argument is greatly strengthened by the numerous analogies connecting the elements ν and κ. It is impracticable at this stage of our inquiries to bring these properly forward; but the reader will hereafter have the conclusion forced upon him that the element ν is, so to say, but a less potent carbon. At any rate, pursuing this train of thought, I have, as I must frankly confess, been brought to the opinion that by far the most probable explanation to be given of the anomalies presented by the densities of the binoxide and tetroxide of nitrogen is that the two gases which pass under this name are not homogeneous gases at all, but in each case are constituted of two gases which, taken together, are made up of the matter of oxygen and nitrogen, but which separately are not so made up. This is undoubtedly a speculation; but it is not a mere speculation, but one founded upon reasonable grounds, which explains difficulties for which no other explanation has ever been propounded, and suggests experiments by which it may be verified or disproved. I proceed to another subject.

The analytical construction of the symbols of the units of matter can be regarded only as preliminary to the consideration of a more complicated problem, namely, how these units are transformed in the processes of chemical change. On this subject there has been much speculation; but it is not going beyond the truth to say that no general theory of the nature of chemical events has yet been devised which will bear the slightest criticism or which is even intelligible. This question also we shall here consider from the analytical point of view afforded by the methods of this Calculus. "The mental," or, as it may be better termed, the "theoretical" analysis of a complex phenomenon into its elements is (it has been truly said) the first step of inductive inquiry*. This theoretical analysis of any object of our study is effected when we replace that thing by some system of things, the result of which, taken together, is equivalent to the result produced by the original object considered. Thus in mechanics the fundamental theorem of the parallelogram of forces is an analytical method, by which it is demonstrated that a force of a given magnitude operating in a given direction may always be theoretically resolved, in an infinite variety of ways, into two or more forces of certain specified magnitudes operating in certain specified directions, the result of which, taken together, is in magnitude and direction identical with the result of the force thus analyzed. Or, again, take the case of the analysis of vibratory movements. Defining the vibration of a point as "motion in a curve which returns into itself with a velocity which is always the same at the same point of the curve," it may be demonstrated that any given vibration of a point in a plane may be analyzed into two component rectilinear vibrations in an infinite variety of ways, and that every vibration of a point, whether plane or not, may be similarly resolved into three component rectilinear vibrations*.

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* J. S. Mill, vol. i. c. vii. 1, p. 437; and also Contents to the same, p. xiv (ed. 1843).

Now precisely as forces are compounded of forces, as vibrations are compounded of vibrations, so chemical events are compounded of chemical events; and from the point of view of this Calculus the theoretical analysis of a chemical event consists in the theoretical determination of a certain special system or systems of such events, the total result of which, as regards the transformations of ponderable matter, is identical with that of the original event.

When we say that a chemical event is thus constituted of other events into which it may be resolved, the question arises, what view we take of the nature of such an event to justify this statement, and of what kind of events it is to be regarded as made up. The reply is given through the peculiar representation of a chemical event, afforded by the method of this Calculus, by which this fundamental conception is suggested. This representation is brought under our notice through the development of the method itself when we express dynamical facts by it, and consider how we are to reason upon them through its instrumentality. I shall therefore treat the subject in this natural order, considering first the construction of the "Organon" or Instrument of reasoning, then the ideas suggested to us by that Instrument, and to which that Instrument is applicable. When the nature of a chemical event has been thus defined so that we clearly see what we have to do, the further inquiry lies before us of the theoretical solution of the problem, namely, given a chemical event, how are we to determine the events of which that event is compounded? This problem may in all cases be solved. The solution is effected by means of a peculiar theorem, which occupies in theoretical chemistry a position analogous to that held in theoretical mechanics by the theorem of the parallelogram of forces. Lastly, I shall give examples of the application of this theorem to actual events and things, when the chemist will have an opportunity of estimating the bearing of this theory upon facts and its practical utility.

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* 'Acoustics, Theoretical,' by W. F. Donkin. Oxford, 1870. Chapter III. On the Composition of Vibrations.

SECTION I.

(1) Our total information as to the identical relation of ponderable matter is comprised in a system of equations constituted upon the principles explained in Part I. Section VI. When we are adequately impressed with this important truth a chemical equation becomes a study of transcendent interest, and we are led to consider in a new light the purport and significance of its algebraical properties. Now even the science of Algebra itself has been defined as consisting but in the "analysis of equations," from which new truths are continually in process of evolution, and to the study of which all other objects of the science may be regarded but as accessory and subordinate. The same is emphatically true of the algebra of chemistry; and the most essential and characteristic feature of the Chemical Calculus, by which it is fundamentally discriminated from other modes of considering the science, is that in it we do not, as in the atomic theory, reason by the intervention of material images, but, setting aside all preconceived ideas, we base our arguments upon the equations themselves, and elicit from them, by the application of algebraical processes, the laws and principles which they implicitly contain.

Now I have been very unwilling to introduce irrelevant matter into a complicated subject; and in the first part of this Memoir, at the risk of some misapprehension, I have entirely confined myself to the consideration of questions the determination of which was essential to the end immediately in view, the "construction," namely, "of chemical symbols." But before proceeding further with the subject, it is necessary to recur to the fundamental principles of the method, and to discuss a question referred to in Part I. (Section IV. (5)), and there postponed, namely, under what conditions the operations of algebraical multiplication and division may be performed upon chemical equations.

The nature of such an equation, and the principles on which such equations are to be constructed, have been fully explained (Part I. Section V.), and it is only necessary to remind the reader that by a chemical equation is here meant an equation of the form

$$v=m\phi+m'\phi_1+m''\phi_2+m'''\phi_3+\ldots=0,$$

where

$$\phi=a^{p}b^{p_1}c^{p_2}\ldots,$$

$$\phi_1=a^{q}b^{q_1}c^{q_2}\ldots,$$

$$\phi_2=a^{r}b^{r_1}c^{r_2}\ldots,$$

$\phi, \phi_1, \phi_2 \ldots$ being the symbols of the units of matter, $a, b, c \ldots$ the symbols of simple weights (Section I. (8)), and $p, p_1, p_2 \ldots q, q_1, q_2 \ldots r, r_1, r_2 \ldots$ positive integers, and $m, m', m'', m''' \ldots$ numerical symbols, positive or negative, satisfying the conditions afforded by the system of indeterminate equations:

$$\begin{aligned} mp+m'q+m''r+ \quad &=0,\\ mp_1+m'q_1+m''r_1+ \quad &=0,\\ mp_2+m''q_2+m'''r_2+ \quad &=0,\\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot \\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot \end{aligned}$$

It will be sufficient to consider the above question in relation to the fundamental equation (Part I. Section IV. (1))

$$xy=x+y, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

which affords the simplest example of a chemical equation. If we proceed to deal with this equation with the same freedom as with a numerical equation, and to multiply each member of this equation, not by a numerical symbol m (which is permissible), but by a chemical symbol v, we have

$$vxy=vx+vy, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

a conclusion which is obviously false; for, interpreting the equation consistently with the principles on which these symbols are constructed, we are informed by it that the unit of matter symbolized as vxy is identical, as regards "weight" or matter, with the aggregate of the two units symbolized as vx and vy; and further, if we apply to equation (2) the principle laid down in equation (1), we have $vxy=v+xy$, $vx=v+x$, $vy=v+y$, and, as a final conclusion, $v=2v$. Again, taking the equation $x^2y=x+xy$, which is true, dividing both members of the equation by the chemical symbol x, we are led to an absurd conclusion, namely, that

$$xy=1+y, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

that is to say, that the matter symbolized as xy is always identical with the matter of an empty unit of space, symbolized as 1, and the matter symbolized as y. But it is only necessary to observe that the equations (2) and (3) to which we are thus led,

$$\begin{aligned} vxy&=vx+vy,\\ xy&=1+y, \end{aligned}$$

are not chemical equations at all, for they do not satisfy the fundamental conditions afforded by the system of indeterminate equations previously referred to,

$$\begin{gathered} mp+m'q+m''r+\ldots 0,\\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot \end{gathered}$$

In short, if we inconsiderately deal with these equations without reference to the principles on which they have been constructed, precisely as though they were numerical equations, we are not only involved in a tissue of absurdities and contradictions, but are compelled to introduce into the system of chemical equations, equations which do not belong to that system at all, and which could never arise from the expression by means of equations of chemical facts.

It is impossible for a false or absurd result to be the consequence of the application to a true statement of a correct principle of reasoning; and we are thus led to the singular conclusion that, although up to a certain point the analogy is perfect between the properties of chemical and numerical symbols, here this analogy terminates and ceases to be a correct guide; and we are apparently forced to admit that in the treatment of chemical equations no processes exist equivalent to the algebraical processes of multiplication and division. Now, first, we may observe that such an imperfect calculus might really exist, logically correct, but with limited methods; for there is nothing either in the nature of symbolical algebra or in the distributive and commutative laws,

$$x(y+z)=xy+xz,$$

$$xy=yx,$$

through which the symbols of chemical operations are related to the symbols of numbers (Part I. Section II. (4) and (5)), to necessitate the application of these processes to equations. Indeed in the actual applications of algebra perfectly similar restrictions are found to exist. The symbols of certain operations do not satisfy the commutative law, and we cannot infer from the equation

$$vxy=vx+vy$$

that (whatever be the interpretation of the symbols)

$$xy=x+y.$$

Thus, for example, in the algebra of logic, while the operation of algebraical multiplication, as applied to equations, is permissible, the performance of the operation of division upon logical equations is absolutely prohibited. Thus we cannot infer from the logical equation

$$zy=zx$$

that $y=x$, which would be tantamount to asserting that because those members of a class y which possess a certain property z are identical with those members of a class x which possess the same property, therefore the class y is identical with the class x [Boole, 'Laws of Thought,' p. 36], an inference obviously erroneous. But even in the algebra of quantity we cannot always perform the operation of division upon equations where such division is formally possible. From the equation $x(x-a)=0$ we cannot infer that $x=0$. The operation of division can only be thus performed by the exclusion of certain symbols upon the assumption that 0 and $\frac{1}{0}$ are not values of the symbol by which we divide. Hence the circumstance that such processes are inadmissible does not detract in any degree from the truth, reality, or logical perfection of an algebraical system. Nevertheless it may be admitted with perfect consistency that a calculus from which such important processes were entirely excluded, and in which the processes of addition and subtraction were the only processes practically available, would be of very limited utility, and could only be regarded as a rudimentary and imperfect system, presenting but a remote resemblance to the extended methods of arithmetical algebra. Such a system is really (although not explicitly and avowedly) presented to us in our actual chemical notation. No algebraical operation has ever as yet been properly performed upon chemical symbols except the operations of addition and subtraction. The great convenience, the brevity and suggestiveness of the notation has led to the universal adoption of the apposition of letters as the expression of what is termed chemical combination. Thus HHO, H_2O, H^2O (for each of these expressions is in use) represent to us the molecule of water as discriminated from the aggregate of hydrogen and oxygen, H+H+O. But these expressions are employed under protest and are algebraically lifeless, for no use is made of them; and, as far as any definite processes of reasoning are concerned in which such symbols are engaged, the molecule of water might with equal advantage have been expressed by the letter A.

(2) We shall now proceed to consider how chemical equations through the application of the principles of this Calculus may be brought into a form adapted to algebraical treatment.

First, it is to be observed that although the processes equivalent to multiplication and division are not universally applicable to chemical equations, yet neither, on the other hand, is it true that these processes are universally inapplicable. There are certain cases in which we may either multiply or divide a chemical equation by a chemical symbol, and the result shall be both interpretable and true. These cases are easily discriminated. Let us take the general equation given, Part I. Section V. (10),

$$v=m\phi+m'\phi_1+m''\phi_2+m'''\phi_3+\ldots=0,$$

and multiply the equation by the factor e^t, where e is a chemical symbol and t a number, we have then

$$me^t\phi+m'e^t\phi_1+m''e^t\phi_2+m'''e^t\phi_3+\ldots=0;$$

and then, in addition to the indeterminate equation

$$\begin{array}{ll} mp+m'q+m''r+ & =0 \\ \cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot & \\ \cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot & \end{array}$$

we have another equation to be satisfied, namely

$$mt+m't+m''t+m'''t+\qquad=0,$$

whence, by dividing by t,

$$m+m'+m''+m'''+\qquad=0.$$

Now if this equation be true, we are led into no error by effecting the multiplication in question. That is to say, we are justified in multiplying a chemical equation by any chemical factor of the form e^t and by any product of such factors, if the sum of the numerical coefficients in that equation be equal to zero, but not otherwise.

Similarly, in the above equation, let ϕ, ϕ_1, ϕ_2, $\phi_3 \ldots$ have a common measure a^t, so that $p=t+s$, $q=t+k$, $r=t+l \ldots$,

$$\begin{aligned}\phi &= a^{t+s} b^{p_1} c^{p_2} \ldots = a^{t+s}\Theta,\\ \phi_1 &= a^{t+k} b^{q_1} c^{q_2} \ldots = a^{t+k}\Theta_1,\\ \phi^2 &= a^{t+l} b^{r_1} c^{r_2} \ldots = a^{t+l}\Theta_2,\\ &\ldots\ldots\ldots\ldots;\end{aligned}$$

then

$$v=ma^{t+s}\Theta+m'a^{t+k}\Theta_1+m''a^{t+l}\Theta_2+\ldots$$

Now if we divide this equation by a^t, we have

$$ma^s\Theta+m'a^k\Theta_1+m''a^l\Theta_2+\ldots=0;$$

and if this equation be true,

$$ms+m'k+m''l+\ldots=0.$$

But we have from the original equation

$$m(t+s)+m'(t+k)+m''(t+l)+\ldots=0;$$

whence, subtracting,

$$m+m'+m''+m'''+\ldots=0.$$

That is to say, we are justified in dividing a chemical equation by any common measure of the symbols by which the equation is expressed of the form a^t if the sum of the numerical coefficients in that equation be equal to zero, but not otherwise. An equation in which this condition is satisfied will be termed a "normal" chemical equation, being an equation to which the rules of algebra are applicable. An equation in which this condition is not satisfied will be termed an "abnormal" chemical equation, being an equation to which these rules cannot be applied.

If we open a chemical treatise and examine the equations expressive of actual results, we cannot fail to be impressed by the circumstance that numerous equations satisfy this condition and are of the "normal" form. For example,

$$2\alpha\chi=\alpha+\alpha\chi^2, \quad \ldots \quad (1)$$

$$\alpha^2\phi\chi^3\xi+3\alpha\xi=3\alpha\chi+\alpha^2\phi\xi^4, \quad \ldots \quad (2)$$

$$\alpha^3\kappa^2\xi+\xi^2=\alpha\xi+\alpha^2\kappa^2\xi^2, \quad \ldots \quad (3)$$

$$2\alpha^2\nu+\alpha^5\kappa^6\xi^4=2\alpha^3\kappa^2\xi+\alpha^3\nu^2\kappa^2\xi^2, \quad \ldots \quad (4)$$

$$\alpha^4\kappa^3\xi^3+3\alpha^2\kappa^2\xi^2=3\alpha\xi+\alpha^7\kappa^9\xi^6, \quad \ldots \quad (5)$$

$$\alpha^3\kappa^3\xi^3+2\alpha\omega=\alpha^3\kappa^3\xi^2+\alpha\xi+\alpha\omega^2, \quad \ldots \quad (6)$$

$$\alpha^7\kappa^6\xi^6+11\alpha\omega=\alpha^7\kappa^6\omega+6\alpha\xi+5\alpha\omega^2, \quad \ldots \quad (7)$$

$$5\alpha+\alpha\omega^2\xi^5=5\alpha\xi+\alpha\omega^2. \quad \ldots \quad (8)$$

These equations have been selected at random. They indicate to us the following facts:—

(1) Two units of hydrochloric acid are identical with a unit of hydrogen and a unit of chlorine.

(2) A unit of oxychloride of phosphorus and three units of water are identical with three units of hydrochloric acid and a unit of phosphoric acid.

(3) A unit of alcohol and a unit of oxygen are identical with a unit of water and a unit of acetic acid.

(4) Two units of ammonia and a unit of oxalic ether are identical with two units of alcohol and a unit of oxamide.

(5) A unit of glycerine and three units of acetic acid are identical with three units of water and one unit of triacetine.

(6) A unit of lactic acid and two units of hydriodic acid are identical with a unit of propionic acid, a unit of water, and a unit of iodine.

(7) A unit of mannite and eleven units of hydriodic acid are identical with a unit of iodide of hexyl, six units of water, and five units of iodine.

(8) Five units of hydrogen and a unit of pentoxide of iodine are identical with five units of water and a unit of iodine.

Such examples might be greatly multiplied. Equations possessing this peculiar property are not due to accident, but to the simplicity of natural laws. In the chemical metamorphoses of which they express the results there is no change of gaseous volume.

It would be by no means difficult thus to make a system of carefully selected equations to which, as satisfying the condition given above, the processes of algebra would be truly applicable. That such a system should be possible, that a system of "normal" equations, subordinate to algebraical laws, should actually be found in the midst, so to say, of an abnormal system, is undoubtedly a most striking and suggestive fact. But nevertheless, so far as any realization of the objects of a calculus are concerned, this circumstance, if taken alone, is totally inoperative. The result of constituting such a system would be to divide chemical equations into two classes—a class with which we could really deal algebraically, and a class with which we could not so deal; and corresponding to these classes we should have, as will presently be seen, two systems of phenomena—a system which we could realize, study, and comprehend, and a second system absolutely unintelligible to us. These anomalies, however, may be completely removed by a mathematical transformation of the equation, to which I must now request the attention of the reader.

(3) It has been remarked (I. Sec. IV. 3), where the meaning and properties of the chemical symbol 1 are under discussion, that any number of numerical symbols 0, 1, 2, 3 may be added to a chemical function without affecting the interpretation of that function as regards "weight." This may be inferred from the fundamental equation $xy=x+y$, which equation becomes if $y=1$, $x=x+1$. It is also an immediate consequence of the interpretation assigned in this Calculus to that symbol, which regarded as a symbol of "weight" (being the symbol of an empty unit of space) is the symbol of "no weight," and regarded as the symbol of an operation is the symbol of (if we may so say) "taking the unit of space as it is" without performing upon it any operation resulting in "weight"

or matter. So that, as regards "weight" or matter, our conception of the "weight" or matter of a unit of hydrogen symbolized as α is precisely the same thing as, and cannot be discriminated from, our conception of the "weight" or matter of a unit of hydrogen and the "weight" or matter of an empty unit of space. Hence as long as "weight" or matter is the only subject under our consideration, we may assert with perfect truth that $\alpha=\alpha+1$, this assertion being strictly analogous to the arithmetical assertion that $1=1+0$. In this peculiar property of the chemical symbol 1 is supplied to us the means of remedying the imperfection of chemical equations and of so changing their form, without affecting their interpretation, as to bring these equations within the recognized domain of algebra. For if in any chemical equation $v=0$ the sum of the numerical coefficients be not equal to zero, it is in our power to make that sum equal to zero, by the addition to the function v of the necessary number of numerical symbols affected by the proper sign; and when this transformation is effected, we may operate upon the equation with confidence and security by every process of algebra—the result being on the one hand greatly to enlarge our powers, and on the other to prohibit inadmissible processes.

Take for example the equation discussed, Part I. Sec. VII. (8),

$$2\alpha^2\nu=3\alpha+\alpha\nu^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

This equation is "abnormal." We should be tempted from the form of the equation to consider the result of dividing by the symbol α, which leads to an equation

$$2\alpha\nu=3+\nu^2,$$

an equation external to the system of chemical equations, the conditions previously referred to not being satisfied in that equation. But this equation, when rendered "normal" on the principles just laid down, is

$$2+2\alpha^2\nu=3\alpha+\alpha\nu^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Now this equation, as regards any assertion made by it in relation to "weight," has precisely the same meaning as the equation from which it is derived; for it is perfectly immaterial whether we say that two units of ammonia are identical as regards "weight" with three units of hydrogen and one unit of nitrogen, or whether we say that two units of ammonia and two "units of space" are thus identical with three units of hydrogen and one unit of nitrogen. But as regards their algebraical properties the equations are fundamentally different; for on the latter equation (2) the performance of the operation of division by the symbol α is impossible, and the result of the transformation of the equation is absolutely to prohibit this operation. But, on the other hand, we may now multiply this equation by any chemical symbol, and the resulting equation will necessarily be interpretable and a proper subject for our consideration. As, for example, if we multiply both sides of the equation by the symbol ξ^2, the equation becomes

$$2\xi^2+2\alpha^2\nu\xi^2=3\alpha\xi^2+\alpha\nu^2\xi^2,$$

an equation in which the requisite conditions are satisfied.

The following examples will sufficiently illustrate the way in which this principle is to be applied for the construction of "normal" equations:—

$$2\alpha+\xi^2=2\alpha\xi, \quad \text{(1)}$$

$$4\alpha^2\phi=6\alpha+\alpha^2\phi^4, \quad \text{(2)}$$

$$\eth+\alpha\chi^2=\eth\alpha\chi^2, \quad \text{(3)}$$

$$\alpha^2\kappa^2+\alpha\beta^2=\alpha^3\kappa^2\beta^2, \quad \text{(4)}$$

$$\alpha\kappa^2\xi^4=\alpha\kappa\xi^2+\kappa\xi^2, \quad \text{(5)}$$

$$\alpha\xi+\kappa\xi=\alpha\kappa\xi^2, \quad \text{(6)}$$

$$\alpha^2\kappa^2\xi^2=\alpha^2\kappa+\kappa\xi^2, \quad \text{(7)}$$

$$\alpha^4\kappa^2\xi^2\nu=2\alpha\xi+\alpha^2\kappa^2\nu, \quad \text{(8)}$$

$$\alpha^2\kappa^2\nu+2\alpha=2\alpha^4\kappa^2\nu, \quad \text{(9)}$$

$$\alpha^2\kappa^2\xi^2\chi+2\alpha^2\nu=\alpha^3\nu\chi+\alpha^3\nu\kappa^2\xi^2, \quad \text{(10)}$$

$$\alpha^3\kappa^4\xi^4+2\alpha^3\phi\chi^5=2\alpha^3\phi\chi^3\xi+2\alpha\chi+\alpha^3\kappa^4\xi^2\chi^2, \quad \text{(11)}$$

$$3\alpha^3\kappa^7\xi+2\alpha^2\nu=\alpha^{10}\nu^2\kappa^{21}+3\alpha\xi, \quad \text{(12)}$$

$$2\alpha^4\kappa^4\xi^2+3\xi^2=2\alpha\xi+2\alpha^3\kappa^4\xi^4, \quad \text{(13)}$$

$$\alpha^3\nu^2\kappa\xi+\alpha\xi+3\alpha\chi^2=\kappa\xi^2+\alpha\nu^2+6\alpha\chi, \quad \text{(14)}$$

$$\alpha^3\nu^2\kappa\xi+2\alpha\nu\xi^2=2\alpha\nu^2+\kappa\xi^2+3\alpha\xi. \quad \text{(15)}$$

The above equations are strictly true, and are the records of actual observations. They are thus to be interpreted:—

(1) Two units of hydrogen and one unit of oxygen are identical with two units of water.

(2) Four units of phosphide of hydrogen are identical with six units of hydrogen and a unit of phosphorus.

(3) A unit of mercury and a unit of chlorine are identical with a unit of protochloride of mercury.

(4) A unit of ethylene and a unit of bromine are identical with a unit of bromide of ethylene.

(5) A unit of oxalic acid is identical with a unit of formic acid and a unit of carbonic acid.

(6) A unit of water and a unit of carbonic oxide are identical with a unit of formic acid.

(7) A unit of acetic acid is identical with a unit of marsh-gas and a unit of carbonic acid.

(8) A unit of acetate of ammonia is identical with two units of water and a unit of aceto-nitrile.

(9) A unit of aceto-nitrile and two units of hydrogen are identical with two units of ethylamine.

(10) A unit of monochloracetic acid and two units of ammonia are identical with a unit of chloride of ammonium and a unit of glycocoll.

(11) A unit of succinic acid and two units of pentachloride of phosphorus are identical with two units of oxychloride of phosphorus and two units of hydrochloric acid and a unit of chloride of succinyl.

(12) Three units of benzoic aldehyde and two units of ammonia are identical with a unit of hydrobenzamide and three units of water.

(13) Two units of butyric acid and three units of oxygen are identical with two units of water and two units of succinic acid.

(14) A unit of urea and a unit of water and three units of chlorine are identical with a unit of carbonic acid, a unit of nitrogen, and six units of hydrochloric acid.

(15) A unit of urea and two units of nitrous acid are identical with two units of nitrogen, a unit of carbonic acid, and three units of water.

These equations, although true, are "abnormal," and cannot be dealt with algebraically.

In the normal form they appear as follows:—

$$2\alpha+\xi^2=2\alpha\xi+1, \quad \ldots \quad (1)$$

$$3+4\alpha^2\phi=6\alpha+\alpha^2\phi^4, \quad \ldots \quad (2)$$

$$\delta+\alpha\chi^2=\delta\alpha\chi^2+1, \quad \ldots \quad (3)$$

$$\alpha^2\kappa^2+\alpha\beta^2=\alpha^3\kappa^2\beta^2+1, \quad \ldots \quad (4)$$

$$1+\alpha\kappa^2\xi^4=\alpha\kappa\xi^2+\kappa\xi^2, \quad \ldots \quad (5)$$

$$\alpha\xi+\kappa\xi=\alpha\kappa\xi^2+1, \quad \ldots \quad (6)$$

$$1+\alpha^2\kappa^2\xi^2=\alpha^2\kappa+\xi^2, \quad \ldots \quad (7)$$

$$2+\alpha^4\kappa^2\xi^2\nu=2\alpha\xi+\alpha^2\kappa^2\nu, \quad \ldots \quad (8)$$

$$\alpha^2\kappa^2\nu+2\alpha=2\alpha^4\kappa^2\nu+1, \quad \ldots \quad (9)$$

$$\alpha^2\kappa^2\xi^2\chi+2\alpha^2\nu=\alpha^3\nu\chi+\alpha^3\nu\kappa^2\xi^2+1, \quad \ldots \quad (10)$$

$$2+\alpha^3\kappa^4\xi^4+2\alpha^3\phi\chi^5=2\alpha^2\phi\chi^3\xi+2\alpha\chi+\alpha^2\kappa^4\xi^2\chi^2, \quad \ldots \quad (11)$$

$$3\alpha^3\kappa^7\xi+2\alpha^2\nu=\alpha^{10}\nu^2\kappa^{21}+3\alpha\xi+1, \quad \ldots \quad (12)$$

$$2\alpha^4\kappa^4\xi^2+3\xi^2=2\alpha\xi+2\alpha^3\kappa^4\xi^4+1, \quad \ldots \quad (13)$$

$$3+\alpha^3\nu^2\kappa\xi+\alpha\xi+3\alpha\chi^2=\kappa\xi^2+\alpha\nu^2+6\alpha\chi, \quad \ldots \quad (14)$$

$$3+\alpha^3\nu^2\kappa\xi+2\alpha\nu\xi^2=2\alpha\nu^2+\kappa\xi^2+3\alpha\xi. \quad \ldots \quad (15)$$

In the preceding examples the equations are rendered "normal" by the arbitrary application of a process, proved to be permissible according to the principles of the method—this process depending upon the circumstances that the interpretation of any chemical function is not (as regards "weight" or matter) affected by the addition to it or subtraction from it of the symbol 1. The application of this principle to the general equation

$$xy=x+y \tag{1}$$

leads to the "normal" form of that equation,

$$1+xy=x+y. \tag{2}$$

(4) We may also arrive at this last equation through the following considerations, which involve some important principles which will hereafter find a further application. Let A, x, y be chemical symbols, and let

$$\mathrm{A}xy+\mathrm{A}ab=\mathrm{A}xb+\mathrm{A}ya;$$

then, from equation (1),

$$\mathrm{A}xy=\mathrm{A}+x+y, \qquad \mathrm{A}xa=\mathrm{A}+x+a,$$
$$\mathrm{A}ab=\mathrm{A}+a+b, \qquad \mathrm{A}yb=\mathrm{A}+y+b.$$

Now putting

$$v=\mathrm{A}xy+\mathrm{A}ab-\mathrm{A}xb-\mathrm{A}ya,$$

and substituting for $\mathrm{A}xy$, $\mathrm{A}ab$, $\mathrm{A}xa$, $\mathrm{A}yb$ the above values,

$$v=0.$$

But we have, from the distributive property of chemical symbols (established in I. Sec. II. 6),

$$v=\mathrm{A}(x-a)(y-b), \tag{1}$$

whence

$$\mathrm{A}(x-a)(y-b)=0;$$

that is to say, the product of any two chemical factors of the forms $x-a$, $y-b$ is necessarily equal to nothing; and the further application of the same reasoning leads to the conclusion that the continued product of any number (not less than two) of factors of this kind is also nothing; that is,

$$\mathrm{A}(x-a)(y-b)(z-c)\ldots=0. \tag{2}$$

Now, since these equations are always true, they are true when A, a, and b are respectively put equal to 1, whence

$$(x-1)(y-1)=0$$

and

$$1+xy=x+y,$$

which is the equation referred to.

Now this equation, regarded as a numerical equation, is satisfied by the values $x=1, y=1$; and since this property is perfectly general, and every symbol of a chemical operation satisfies this condition, every chemical equation must necessarily be true when the prime factors by which it is expressed are severally or collectively put equal to 1.

(5) Now if this principle be applied to an abnormal equation, we are led to the assertion (Part I. Sec. IV. (3))

$$1(1)=2(1)=3(1)\ldots=n(1)=0;$$

or, suppressing the chemical symbol (1), $1=2=3\ldots=n=0$, which assertion is indeed not inconsistent with the use and interpretation of the chemical symbol (1), but is entirely at variance with the numerical interpretation of that symbol, so that the greatest possible divergence is apparently exhibited between the use which is made in this Calculus of that symbol and its arithmetical signification*. But if this same substitution be effected in a "normal" equation the anomaly disappears. No divergence is found between the arithmetical and chemical use of the symbol 1, and the equation is true *both* as an arithmetical and chemical equation; for from the very principle of its construction every "normal" equation must necessarily vanish when the prime factors in that equation are severally put equal to 1. So that the effect of making this substitution is simply to lead us to the assertion that $0=0$, which is an arithmetical as well as chemical truth; that is to say, every "normal" equation is true, not only as a "chemical equation," but also as an arithmetical equation for the only arithmetical value which the symbols can possibly assume, namely the value 1; and when this value is assigned to every prime factor by which the equation is expressed, the chemical equation is turned into an arithmetical equation, which is both interpretable and true. The reason of this is that the chemical symbol 1 is absolutely identical, as regards its algebraical properties, with the arithmetical symbol 1, although having a totally different interpretation from that symbol, a point which has been fully demonstrated and discussed in Part I. Sec. III., and also that a "normal" equation is subject to precisely the same rules of algebraical treatment as an arithmetical equation, as has been just demonstrated.

The real and valid character of the preceding reasoning will be evident from the following example, which illustrates the way in which a chemical function passes into a numerical function when chemical symbols assume the numerical value 1, and the inferences which may be thus drawn.

(6) In the first part of this memoir the symbols of the units of chemical substances are determined by the aid of certain indeterminate numerical equations, at which we arrive by the direct application to chemical equations of the fundamental properties of chemical symbols so frequently referred to $xy=x+y$ (I. Sec. VII.).

Take, for example, the equation given, Part I. Sec. VII. (8),

$$2\alpha^{m}\upsilon^{m_1}=3\alpha+\alpha^{n}\upsilon^{n_1};$$

from this equation we are able immediately to infer that

$$(\alpha^{m}\upsilon^{m_1})^2=\alpha^3.\,\alpha^{n}\upsilon^{n_1},$$

whence

$$2m=3+n, \qquad 2m_1=n_1.$$

---

* The apparent paradox involved in this assertion may be removed by assigning a special symbol, ϵ^p, to the unit of space, p being a positive integer. But on investigating the properties of this symbol we should soon find that $\epsilon=\epsilon^p$, whatever be the value of p (the value 0 included), and that, as we might always replace algebraically the symbol ϵ^p by the symbol (1), we were really dealing with the symbol 1 under another name. Such paradoxes, however, have no significance when the meaning of the expressions employed is properly understood. Thus De Morgan, in his 'Double Algebra' (ed. 1849, p. 114), speaking of the term addition as there employed by him, says, "*Nor is there, in one sense, the slightest objection to saying that* 12 *and* 12 *make* 10," an assertion quite as paradoxical (to say the least) as any here made.

But we may also proceed thus. Reducing the above equation to the "normal" form, we have

$$2+2\alpha^{m}\nu^{m_1}=3\alpha+\alpha^{n}\nu^{n_1}.$$

Putting $\nu=1$, we have

$$2+2\alpha^{m}=3\alpha+\alpha^{n},$$

whence . . . .

$$\frac{\alpha(2\alpha^{m-1}-\alpha^{n-1}-1)}{\alpha-1}=2.$$

Since this equation is always true, it is true where $\alpha=1$. But the numerical value of the left-hand member of this equation, when $\alpha=1$, is $2(m-1)-(n-1)$; whence we have

$$2m-n-1=2,$$

and, as before,

$$2m=3+n;$$

again, putting $\alpha=1$, we have

$$2+2\nu^{m_1}=3+\nu^{n_1}$$

and

$$\frac{\nu^{m_1}-\nu^{n_1}}{\nu^{m_1}-1}=-1.$$

But the numerical value of $\frac{\nu^{m_1}-\nu^{n_1}}{\nu^{m_1}-1}$, when $\nu=1$, is $\frac{m_1-n_1}{m_1}$; whence we have, as before,

$$2m_1=n_1.$$

We thus arrive by the consideration of the numerical value of chemical symbols at the results previously inferred by an apparently different process. But the truth is that the very same principles are really employed in both methods, which differ only in the stage at which those principles are introduced.

The process here employed for the reduction of a chemical equation to the "normal" form, with the view of rendering that equation amenable to algebraical treatment, is strictly in conformity with the spirit of algebraical methods. The ordinary artifice for the solution of a quadratic equation is based upon a somewhat similar principle. A particular form of the symbol 0 is there added to the function, by which its value and interpretation are not affected, but which enables us to express the function by means of factors. But an illustration, perhaps still more in point, is the process familiar to mathematicians, by which an equation is rendered "homogeneous," which is effected by the introduction of an arbitrary factor with which we work precisely as with one of the real factors of the symbols which appear in the equation, and which, at the conclusion of the algebraical operations, to which the equation is submitted, is put equal to 1 and suppressed.

(7) It is, however, important to notice that although it is strictly true that if the

assertion made in the symbol = be limited to identity of "weight," $x=x+1$; so that, as regards this identity, the assertion made in the "normal" equation

$$1+2\alpha\xi=2\alpha+\xi^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

is the very same assertion as that made in the "abnormal" equation

$$2\alpha\xi=2\alpha+\xi^2; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

it being perfectly immaterial, as before remarked, whether we say that the "weight" of a unit of space and the "weight" of two units of water is identical with the "weight" of two units of hydrogen and a unit of oxygen, or whether we simply say that the "weight" of two units of water is identical with the "weight" of two units of hydrogen and a unit of oxygen, just as it is immaterial whether we say that three and two are identical with five, or that nothing and three and two are identical with five, the selection among such expressions being dictated merely by algebraical convenience. Yet nevertheless a very real and appreciable distinction may be drawn between the two assertions, which from another aspect have by no means a strictly equivalent meaning. The former equation (1) necessarily includes every assertion which is made in the latter; but the latter (2) does not include every assertion which is made in the former. For if we extend our notion of identity to identity of "space" as well as of "matter," it is open to us to assign an interpretation to the symbol of identity = in equation (1) which cannot be assigned to that symbol in equation (2). A unit of space and two units of water are identical, both as regards "weight" and as regards the "space" occupied by that "weight," with two units of hydrogen and a unit of oxygen. But it is not true that two units of water are identical, both as regards "weight" and as regards the "space" occupied by that "weight," with two units of hydrogen and a unit of oxygen; for the space occupied by two units of water is 2000 cub. centims., but the space occupied by two units of hydrogen and a unit of oxygen is 3000 cub. centims. [I. Sec. I. (10, 11)]. Thence, in the reduction of a chemical equation to the "normal" form, we not only render the equation algebraically correct, but also extend and complete the idea expressed in the equation.

Now this conception of a perfect and complete identity, both as regards "space" and matter, cannot arise so long as we confine our inquiries to the composition of the units of ponderable matter, as considered in the first part of this Calculus, but has its origin exclusively in those dynamical inquiries which relate to the changes of matter. For this there is an obvious reason. In investigating the laws of gaseous combinations, which are the special objects of this method, a most essential point to be considered, whether from the point of view of the chemist or of the physical philosopher, is the relation of "matter" to "space;" and in the adequate consideration of a chemical change we are bound to take cognizance not only of changes in the composition of the "units of ponderable matter," but also of changes in the "bulk" of matter, and to find the means of entering these changes in our equations. The contrivance by which a chemical equation is rendered "normal" is a method of effecting this entry.

(8) According to the reasoning previously pursued we arrive at the numerical equation $2m=3+n$, $2m=n$, by successively assigning to the chemical symbols α, ν the value 1, a value common to numerical and chemical symbols; but this method is but an illustration of a wider principle. We may lay down the rule, that if throughout a chemical equation we substitute the symbol of any one "simple weight" for any other "simple weight," that equation will still be true. This statement does not mean that the symbols of all simple weights have the same meaning, and that it is indifferent which we employ, or that the assertion made in the equation thus modified is the same as the assertion made in the original equation, but that if the original equation be true, the new assertion made in the equation thus modified is also true. We may compare the two members of a chemical equation to the two pans of a balance which is kept in equilibrium by a system of weights of 1 ounce, 2 ounces, 3 ounces, 1 lb., 2 lbs., 3 lbs., and the like in each pan, the same number of weights of each kind being in the two pans respectively. The equilibrium of the balance is not only unaffected by any change in the distribution (or arrangement) of the weights in each pan, but is also unaffected by the simultaneous removal from each pan of all the weights of the same kind, or by substituting weights weighing 1 lb. for the weights weighing 1 ounce, or any analogous substitutions. At first sight it would appear that the chemical symbol 1 should be excluded from the operation of this rule; for it is not the case that in the normal equation

$$1+xy=x+y,$$

where the symbol 1 appears we may write x (although where x appears we may write 1), and that the resulting equation should still be true. The reply to this difficulty is that, in order to arrive at the above equation, we have tacitly assumed the truth of the equation $1^p=1$, and that the equation in the above form is really imperfect. If, however, we render the equation homogeneous, remembering that $\varpi^p=1$, whatever be the value of p, we have for the complete equation

$$\varpi^2+xy=x\varpi+y\varpi,$$

to which the rule is applicable.

SECTION II.—ON SIMPLE AND COMPOUND EVENTS.

(1) Through the transformation of equations, described in the last section, the Chemical Calculus is finally constituted as a symbolical method adapted to algebraical reasoning. I shall now proceed to consider, in the light of these principles, the nature of the fundamental conceptions through which we are to reason as to the phenomena of chemical change. Here, again, pursuing the method employed in the first part of this Calculus, I shall endeavour to assign a precise meaning to the terms employed, and accurately to define those conceptions. But in this case the notions themselves are so

mixed up with the symbolical forms in which they originate, that it would be inexpedient and indeed almost impracticable, to attempt to explain these conceptions except in reference to these forms, and these definitions will be given as the occasion arises.

A chemical equation has been hitherto regarded in this Calculus only as an assertion in regard to the identity of certain units of ponderable matter. But there is another and equally just point of view from which such an equation may be considered, namely, as the record of a "chemical event" or "metamorphosis;" and a chemical equation of the form $u=0$ may correctly be termed the symbol of such an event. For from the very principle on which such equations are constructed, the assertion that A is identical as regards "weight" with B necessarily implies that A has been chemically converted into B, or that B has been chemically converted into A, for otherwise we could not possibly have been aware of this identity, our only knowledge of the identical relation of matter being derived, as has already been fully explained (I. Sec. VI.), from the chemical transmutation of matter. And when we write down such equations as those given in the last section (Sec. I. (2)(3)), we are recording the various events of which we have become cognizant in the history of chemical metamorphosis.

The term "chemical event" will be here employed to denote the occurrence of any change whatever in the chemical composition of the units of matter of which the result is or may be expressed in an equation; and precisely as we speak of a "chemical operation" as the "simple weight" which is the result of that operation, so also may we refer to a "chemical equation" as the "chemical event" of which it symbolizes the result, and deal with it as representing the event itself. Such an event is here represented to us as in the strictest sense a "metamorphosis" or "change of form;" not that we venture to offer any material image, picture, or physical representation of that as yet inscrutable phenomenon, but we speak of it in this language because that change is indicated to us and adequately expressed by changes in the arrangement and structure of these symbolic forms, through which it is brought under the cognizance of our intelligence.

(2) Now just as the algebraical sum or aggregate (as we shall term it) of two or more equations itself constitutes an equation, so every collection or aggregate of chemical events constitutes an event. This is true whether those events are considered as occurring successively or simultaneously. An event of which the result is expressed by the algebraical sum of two or more equations will be termed "an aggregate," and will be spoken of as "the aggregate" of those two or more events expressed in those equations, and will be said to be "constituted" of those events. We are hence led to discriminate chemical events as "simple" and "compound" events.

Definition.—A "compound" event is an event which is regarded in the system of events under our consideration, whatever that may be, as "constituted" of two or more events, and "a simple event" is an event which in that system is not so regarded. But these terms necessarily have reference to some special mode of regarding and considering the phenomena, and apart from such considerations the terms are unmeaning;

for there is no property of a chemical phenomenon which compels us to regard it either from the one or the other point of view. In this respect the use made of the terms "simple event" and "compound event" is analogous to the use made in the first part of this memoir of the terms "simple weight" and "compound weight," a division coextensive with that into "distributed" and "undistributed" weights (I. Sec. I. 7, 8, 12). For not only are these terms dependent upon one another, a "simple weight" being defined as a "weight" which is not "compound," but they also have reference to some special system of phenomena considered from a definite point of view—a "distributed" weight being a weight which *in such a system of events* is resolved into two or more weights, or made up from such weights, and an "undistributed" weight being a weight which *in the same system of events* is *not* so resolved or so made up," there being nothing whatever in the properties of matter, apart from such considerations, to justify the use and application of such terms at all. Hence a "weight" which from one point of view and in one system of events is regarded as a "compound" and "distributed" weight, from another point of view and in another system of events may, with equal reason, be regarded as a "simple" or "undistributed" weight. These conceptions have their origin in the consideration of the chemical properties of matter, but may be transferred without any fundamental alteration of meaning from matter to phenomena, or "the changes of matter"—the symbol of a compound event being the symbol of one event which is "an aggregate" or collection of some other events, precisely as the symbol of the unit of hydrochloric acid $\alpha\chi$ is the symbol of one operation which is a combination of the two operations α and χ. As it is essential that a correct appreciation should be made of what is here meant by a compound event, I shall proceed to give some examples of such events.

Example (1).—Let us suppose ourselves to be informed that a unit of binoxide of hydrogen is identical with a unit of hydrogen and a unit of oxygen, that is to say, that

$$1+\alpha\xi^2=\alpha+\xi^2; \quad \ldots \ldots \quad (1)$$

this information could only be derived from the circumstance that the event of the transformation of a unit of binoxide of hydrogen into a unit of hydrogen and a unit of oxygen had, in some system of chemical metamorphoses, actually occurred, and the above equation informs us of all the circumstances of that event (as regards the identities of matter and space) which the event involves. Now if no other information than this, either real or theoretical, be laid before us, we can form but one judgment as to that event, namely, that it is a "simple event;" for we are supplied with no information which enables us to regard it as a "compound event," and a "simple event" is an event which, in relation to our information, is *not* compound. But let us also be informed of the two following circumstances, namely, that two units of binoxide of hydrogen are identical with two units of water and a unit of oxygen, and also that two units of water are identical with two units of hydrogen and a unit of oxygen. The events whence this information was derived are thus recorded:—

$$1+2\alpha\xi^2=2\alpha\xi+\xi^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$1+2\alpha\xi=2\alpha+\xi^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

It is now in our power to take a different view of the nature of the event,

$$1+\alpha\xi^2=\alpha+\xi^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

For if we add together the right-hand and left-hand members of equations (2) and (3) and divide the result by 2, we constitute equation (1); that is to say, this event (1) may be regarded as a compound event, which is the aggregate of the two events,

$$\tfrac{1}{2}+\alpha\xi^2=\alpha\xi+\tfrac{1}{2}\xi^2,$$

$$\tfrac{1}{2}+\alpha\xi=\alpha+\tfrac{1}{2}\xi^2;$$

for not only is equation (1) an algebraical inference from equations (2) and (3), but it is actually in our power to effect the chemical transformation of a unit of binoxide of hydrogen into a unit of hydrogen and a unit of oxygen by effecting two several transformations, namely, the transformation of a unit of binoxide of hydrogen into a unit of water and half a unit of oxygen, and also the transformation of a unit of water into a unit of hydrogen and half a unit of oxygen; and the result of these events (whether they occur successively or simultaneously, or in whatever order) is the transformation of a unit of binoxide of hydrogen into a unit of hydrogen and a unit of oxygen, and no other occurrence or event—it being quite immaterial as regards the result whether we first transform a unit of binoxide of hydrogen into a unit of water and half a unit of oxygen, and then take that unit of water and resolve it into a unit of hydrogen and half a unit of oxygen, or whether we begin by resolving a unit of water into a unit of hydrogen and half a unit of oxygen, and then resolve a unit of binoxide of hydrogen into a unit of water and half a unit of oxygen, or whether the two events occur together. In each case the final result is precisely the same, namely, the conversion of a unit of binoxide of hydrogen into a unit of hydrogen and a unit of oxygen, and nothing else.

Now as the equation

$$1+\alpha\xi^2=\alpha+\xi^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

is an inference which may be arrived at in many ways and from many systems of equations, so may we regard this event as "constituted" of other "events" in many ways. Thus, for example, a unit of chlorine and a unit of binoxide of hydrogen are identical with two units of hydrochloric acid and a unit of oxygen, and also two units of hydrochloric acid are identical with a unit of chlorine and a unit of hydrogen, whence

$$1+\alpha\chi^2+\alpha\xi^2=2\alpha\chi+\xi^2,$$

$$2\alpha\chi=\alpha+\alpha\chi^2.$$

The event (1) is also an aggregate of these two events. But these events, of which event (1) is constituted, may themselves be compound events, and that in various ways. Thus, for example, the event

$$1+\alpha\chi^2+\alpha\xi^2=2\alpha\chi+\xi^2$$

may be considered as the aggregate of the two events,

$$2\alpha\chi+\alpha\xi^2=\alpha\chi^2+2\alpha\xi,$$
$$1+2\alpha\chi^2+2\alpha\xi=4\alpha\chi+\xi^2;$$

in which case the event (1) will be the aggregate of three events—

$$2\alpha\chi+\alpha\xi^2=\alpha\chi^2+2\alpha\xi,$$
$$1+2\alpha\chi^2+2\alpha\xi=4\alpha\chi+\xi^2,$$
$$2\alpha\chi=\alpha+\alpha\chi^2.$$

Example (2).—Again, a unit of ammonia and three units of iodide of ethyl are identical with three units of hydriodic acid and a unit of triethylamine.

This event is thus expressed:

$$\alpha^2\nu+3\alpha^3\kappa^2\omega=3\alpha\omega+\alpha^8\kappa^6\nu.$$

Now this event may be regarded as the aggregate of the three following events:—

$$\alpha^2\nu+\alpha^3\kappa^2\omega=\alpha\omega+\alpha^4\kappa^2\nu,$$
$$\alpha^4\kappa^2\nu+\alpha^3\kappa^2\omega=\alpha\omega+\alpha^6\kappa^4\nu,$$
$$\alpha^6\kappa^4\nu+\alpha^3\kappa^2\omega=\alpha\omega+\alpha^8\kappa^4\nu.$$

Each of these events, too, may itself be regarded as a compound event; thus the event

$$\alpha^2\nu+\alpha^3\kappa^2\omega=\alpha\omega+\alpha^4\kappa^2\nu$$

may be considered as constituted of the following events:—

$$\alpha^3\kappa^2\omega+\alpha\kappa\nu\xi=\alpha^3\kappa^3\nu\xi+\alpha\omega, \quad \text{. . . . .} \quad \text{(a)}$$
$$\alpha^3\kappa^3\nu\xi+\alpha\xi=\alpha^4\kappa^2\nu+\kappa\xi^2, \quad \text{. . . . .} \quad \text{(b)}$$
$$\kappa\xi^2+\alpha^2\nu=\alpha\xi+\alpha\kappa\nu\xi. \quad \text{. . . . .} \quad \text{(c)}$$

For if (a) we take a unit of iodide of ethyl and a unit of cyanic acid and transform them into a unit of hydriodic acid and a unit of cyanic ether, and then (b) take a unit of cyanic ether and a unit of water and transform these into a unit of ethylamine and a unit of carbonic acid, and also (c) take a unit of ammonia and a unit of carbonic acid and transform these into a unit of water and a unit of cyanic acid (3), the final result of these successive events will be to transform a unit of ammonia and a unit of iodide of ethyl into a unit of hydriodic acid and a unit of ethylamine, which is the event in question.

Example (3).—The following equation expresses to us the transformation of two units of marsh-gas and a unit of oxygen into two units of methylic alcohol,

$$2\alpha^2\kappa+\xi^2=2\alpha^2\kappa\xi+1.$$

This event may be regarded as an aggregate of seven events, as follows:—

$$\begin{gathered}
\alpha^2\kappa + \alpha\chi^2 = \alpha^2\kappa\chi + \alpha\chi, \\
\alpha^2\kappa + \alpha\chi^2 = \alpha^2\kappa\chi + \alpha\chi, \\
\alpha^2\kappa\chi + \alpha\xi = \alpha^2\kappa\xi + \alpha\chi, \\
\alpha^2\kappa\chi + \alpha\xi = \alpha^2\kappa\xi + \alpha\chi, \\
2\alpha + \xi^2 = 2\alpha\xi + 1, \\
2\alpha\chi = \alpha + \alpha\chi^2, \\
2\alpha\chi = \alpha + \alpha\chi^2,
\end{gathered}$$

and has absolutely been realized by the synthesis of these events. But each of these events may itself be an aggregate. Take, for example, the event

$$\alpha^2\kappa\chi + \alpha\xi = \alpha^2\kappa\xi + \alpha\chi\,;$$

this result may be obtained by the synthesis of the two following events:—

$$\alpha^2\kappa\chi + \alpha^2\kappa^2\xi^2 = \alpha\chi + \alpha^2\kappa^2\xi^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(a)}$$

$$\alpha^2\kappa^2\xi^2 + \alpha\xi = \alpha^2\kappa^2\xi^2 + \alpha^2\kappa\xi. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(b)}$$

For if we take (a) a unit of chloride of methyl and a unit of acetic acid and transform them into a unit of hydrochloric acid and a unit of acetate of methyl, and then (b) take that unit of acetate of methyl and a unit of water and transform them into a unit of acetic acid and a unit of methylic alcohol, the final result of these successive transformations will be the transformation of a unit of chloride of methyl and a unit of water into a unit of alcohol and a unit of hydrochloric acid.

Example (4).—Again, let the event considered be the synthetical construction of methylic alcohol from carbon, hydrogen, and oxygen according to the following equation,

$$2\kappa + 4\alpha + \xi^2 = 2\alpha^2\kappa\xi + 5. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Now this event may be regarded as an aggregate of the three following events, the first of which is the "aggregate" considered in the last example, the latter events being repeated twice,

$$(2) \qquad 2\alpha^2\kappa + \xi^2 = 2\alpha^2\kappa\xi + 1,$$

$$(3) \qquad 2\alpha + \kappa = \alpha^2\kappa + 2.$$

Again, the second of these two events may be regarded as constituted of the following events:—

(1) Formation of acetylene from carbon and hydrogen,

$$2\kappa + \alpha = \alpha\kappa^2 + 2.$$

(2) Formation of olefiant gas from acetylene and hydrogen,

$$\alpha\kappa^2 + \alpha = \alpha^2\kappa^2 + 1.$$

(3) Formation of alcohol from olefiant gas and water,

$$\alpha^2\kappa^2+\alpha\xi=\alpha^3\kappa^2\xi+1.$$

(4) Oxidation of alcohol with the formation of acetic acid and water,

$$\alpha^3\kappa^2\xi+\xi^2=\alpha\xi+\alpha^2\kappa^2\xi^2.$$

(5) Decomposition of acetic acid into marsh-gas and carbonic acid,

$$1+\alpha^2\kappa^2\xi^2=\alpha^2\kappa+\kappa\xi^2.$$

(6) Resolution of carbonic acid into carbon and oxygen,

$$1+\kappa\xi^2=\kappa+\xi^2.$$

For if we write down these equations in order, and add the corresponding members of those equations, we arrive at the equation required,

$$2\kappa+\alpha=\alpha\kappa^2+2,$$
$$\alpha\kappa^2+\alpha=\alpha^2\kappa^2+1,$$
$$\alpha^2\kappa^2+\alpha\xi=\alpha^3\kappa^2\xi+1,$$
$$\alpha^3\kappa^2\xi+\xi^2=\alpha\xi+\alpha^2\kappa^2\xi^2,$$
$$1+\alpha^2\kappa^2\xi^2=\alpha^2\kappa+\kappa\xi^2,$$
$$1+\kappa\xi^2=\kappa+\xi^2.$$

Now we have seen in the last example that the event (2) may be considered as constituted of 8 events, and the event (3) as constituted of 6 events, each of which is repeated twice in the "compound event" (1), which therefore is to be regarded as constituted of 20 distinct "occurrences" or "events." This last example is derived, with certain modifications, from the work of the distinguished chemist BERTHELOT, 'Chimie organique fondée sur la Synthèse,' where numerous similar illustrations of the aggregation of phenomena may be found. "C'est ainsi," says BERTHELOT, "que l'on parvient à la synthèse d'un alcool par une succession régulière de réactions définies" (Chimie organique fondée sur la Synthèse, ed. 1860, vol. i. p. 97).

The synthesis of an alcohol thus effected by BERTHELOT is undoubtedly brought about by the aggregation of phenomena, and so far affords an illustration of a compound event. But this aggregate of phenomena is an accidental collection of events selected to attain a particular end, which, although concurring to produce the result in question, are yet marked by no regular order or sequence and are characterized by no common properties. Such aggregates are not to be confounded with the aggregates subsequently considered in this Calculus.

SECTION III.—ON THE "CAUSES" OF CHEMICAL EVENTS.

Having thus arrived at the conception of the nature of that complex phenomenon which is here termed "a compound chemical event" as an aggregate or collection of events which concur to produce a specified result, and that result alone, we shall endeavour to arrive at a similar comprehension of the nature of those operations by which events occur, that is to say, of "the causes of events." The fundamental idea through which these operations will be conceived of in this Calculus is given in the following definition.

(1) Definition :—

If in any chemical event the change in the arrangement of the symbols by which the composition of the units of matter before and after the event respectively is symbolized be of such a nature that where in the arrangement before the event the symbol x appears the symbol a appears in the arrangement after that event, and where the symbol a appears in the arrangement before that event the symbol x appears in the arrangement after that event (x and a being symbols of two among the prime factors by which the equation is expressed), so that the two arrangements differ in this respect, and in this respect alone, then that event is said to occur by the "substitution" or "exchange" of a for x, and that "substitution" is said to be the "cause" of that event.

It is evident that, consistently with this definition, the same events may arise from more than one "cause;" for it may be true that where, in the arrangement before the event, the symbol x appears the symbol a appears in the arrangement after that event, and that where the symbol x appears in the arrangement after that event the symbol a appears in the arrangement before that event, so that the two arrangements differ in this respect, and in this respect alone; but it may also be true that where in the arrangement before the event the symbol y appears the symbol b appears in the arrangement after that event, and where the symbol b appears in the arrangement before the event the symbol y appears in the arrangement after the event, so that the two arrangements differ in this respect, and in this respect alone. In this case the event may be referred to *either* of *two* "causes," namely, to the substitution of a for x or for b for y. These causes represent two alternative hypotheses as to the ways in which the event may have occurred. Similarly one and the same "event" may be referred to several alternative "causes," representing various hypotheses as to the modes of the occurrence of that event.

(2) If, however, the two arrangements so differ that where the symbol x appears in the arrangement before the event a appears in the arrangement after the event, and where a appears in the arrangement before the event x appears in the arrangement after the event, and also differ in the circumstance that where the symbol y appears in the arrangement before the event b appears in the arrangement after the event, and where b appears in the arrangement before the event y appears in the arrangement after the event, then that event is said to occur by two substitutions, namely, by the substitution of a for x and of b for y; and these two substitutions are said to be the two "causes" of that event for the production of which they concur; and, similarly, if the arrangements referred to differ in the n circumstances that where $x, y, z \ldots$

appear in the arrangement before that event, $a, b, c \ldots$ appear in the arrangement after that event, and where $a, b, c \ldots$ appear in the arrangement before that event, $x, y, z \ldots$ appear in the arrangement after that event, then the event is said to occur by the n substitutions of a for x, b for y, c for z, and these substitutions are termed the n causes of the event which concur for its production.

The same remark is applicable here as in the case of an event occurring by a single "cause," namely, that as events may occur by a single "cause" in any number of ways, so also may we have events occurring by a set of two or more "causes" in any number of ways, each set of "causes" representing an alternative hypothesis as to the mode of the occurrence of that event.

(3) Definition:—

If an event be regarded as occurring by the substitution of a for x, x is termed the symbol of the "variable" in that event, and a is termed a "value" of x (being that for which x is exchanged); if the event be regarded as occurring by the substitutions of a for x, b for y, c for z, x, y, z are termed the symbols of the "variables" in that event, and a, b, c the "values" of those "variables," and so on for any number of substitutions.

(4) Definition:—

A "constant weight" is a weight which, in any specified event or system of events, is *not* exchanged for any other weight.

(5) It is a consequence of the above definitions that the "substitutions" by which events occur will be indicated to us by certain algebraical properties of the equations which express the results of those events. Thus, if an event occurs by the substitution of a for x, it is evident, from the definition, that the equation corresponding to the event must vanish when x is put equal to a; and if the event occurs by one substitution, and in one way only by such a substitution, so that it can be referred to one cause alone, the equation will vanish under this condition, and under no other similar condition. If there be two alternative "causes" of the event, so that the event may be regarded as occurring either by the substitution of a for x, or of b for y, the equation will vanish when x is put equal to a, and also will vanish when y is put equal to b; and if there be n alternative causes of the event, so that the event may occur either by the substitution of a for x, of b for y, of c for $z, \ldots$ the equation will vanish in n ways, namely, when either x is put equal to a, or y is put equal to b, or z is put equal to $c, \ldots$ If the event may be referred to two "causes" which concur for its production, so that it occurs by two substitutions, namely, the substitutions of a for x and of b for y, the equation will vanish if in that equation we simultaneously put x equal to a and y equal to b; and if the event may be referred to n "causes" which concur for its production, so that it occurs by n substitutions of a for x, b for y, c for $z \ldots$, the equation will vanish if in that equation we simultaneously put x equal to a, y equal to b, z equal to $c \ldots$ And, similarly, if the equation may not only be referred to n causes, but may be referred in m ways to n causes, there will then be m ways in which the equation will vanish under a similar condition.

It is necessary to notice that every algebraical condition under which a chemical equation may vanish does not necessarily correspond to some one among the "causes" of the event, or throw light upon such causes. For an equation may vanish under algebraical conditions which are chemically uninterpretable and unmeaning. But if an equation vanish under certain specified conditions, such as that $x=a$, when x and a are two of the prime factors of an equation constructed upon the principles before laid down (I. Sec. V., II. Sec. I.), these conditions are capable of interpretation, and inform us of the "real" causes of the event—the term "real" causes being here used to distinguish such causes from "imaginary" causes, which may be defined as substitutions corresponding to algebraical conditions which are uninterpretable. It is quite possible that the consideration of "imaginary" causes may hereafter find its place in the chemical Calculus and lead to true results, but we shall not now consider them.

(6) I shall now proceed to the consideration of certain forms of equations corresponding to the events of which the definition has now been given, and the interpretation of those forms.

I will commence with the equation already referred to (Part II. Sec. I. (4)),

$$\mathrm{A}xy+\mathrm{A}ab=\mathrm{A}ya+\mathrm{A}xb.$$

Now, if we compare the arrangement before the event, namely, $\mathrm{A}xy+\mathrm{A}ab$, with the arrangement after the event, namely, $\mathrm{A}ay+\mathrm{A}xb$, superposing those arrangements the one upon the other, thus—

$$\mathrm{A}xy+\mathrm{A}ab, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad \mathrm{I.}$$
$$\mathrm{A}ay+\mathrm{A}xb, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad \mathrm{II.}$$

it will be seen that where x appears in the arrangement before the event, a appears in the arrangement after the event, and where x appears in the arrangement after the event, a appears in the arrangement before the event, and the two arrangements differ in this respect, and in this respect alone. Thence, according to the definition, the event symbolized in the above equation may occur by the substitution of a for x, and this substitution is a cause of that event.

But, since $\mathrm{A}ay+\mathrm{A}xb=\mathrm{A}xb+\mathrm{A}ay$, we may compare these arrangements from another point of view; for, writing the arrangements after the event as $\mathrm{A}xb+\mathrm{A}ay$, and again, as before, superposing the arrangements the one upon the other, thus— .

$$\mathrm{A}xy+\mathrm{A}ab, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad \mathrm{I.}$$
$$\mathrm{A}xb+\mathrm{A}ay, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad \mathrm{II.}$$

it will be seen that where y appears in the arrangement before the event (I.), b appears in the arrangement after the event (II.), and where b appears in the arrangement before the event (I.), y appears in the arrangement after the event (II.), and the two arrangements differ in this respect, and in this respect alone. Whence, according to the definition, the event may occur by the substitution of b for y, which substitution is a real "cause" of that event.

This event therefore is an event which may be referred to either of two "causes," namely, to the substitution of a for x and of b for y, which "causes" represent two alternative hypotheses as to the ways of the occurrence of the event.

But, further, not only is it true that the event may be referred to one or the other of these two causes, the occurrence of either of which is sufficient to account for the event, but the event must be so referred as there is no other substitution whatever which will produce the result in question. This may be proved experimentally by altering the arrangement of the letters in every permissible manner, and, as before, superposing the arrangements.

But it is sufficient to observe that there are only two conceivable cases; for where Axy appears before the event, either Aay appears after the event or Axb so appears. In the former case the result is attained, if possible at all, by the substitution of a for x; in the latter case, by the substitution of b for y.

We might also, in demonstrating the above proposition, have reasoned thus: having proved that the event may occur by the substitution of a for x, since in the above equation we may change the places of x and y, provided also we change the places of a and b, and the equation will be unaffected by this alteration. Hence, whatever is true of x and a is also true of y and b; that is to say, whatever assertion may be made in reference to that equation in regard to x and a is also true if in that assertion we substitute y for x and b for a.

Writing the above equation in the form $u=0$, we have, from the distributive law (Part I. Sec. II. (6)),

$$u=A(x-a)(y-b),$$

from which it is apparent that the substitution by which the event occurs, that is to say, the "causes" of the event, are indicated to us by the factors of this equation, and are coincident with those factors; and, precisely as the factors in a numerical equation of this form indicate to us the two sole conditions under which the numerical identity asserted in the equation is possible, namely, the identity of x and a, or the identity of y and b, so in the case of this chemical equation the factors indicate to us the two sole conditions, or hypotheses, under which the chemical identity expressed in that equation is possible, namely, the substitution of a for x, or the substitution of b for y. For the only possible way in which the "weights," or matter, symbolized as $Axy+Aab$ can have been chemically converted into the "weights," or matter, symbolized as $Axb+Aay$ is by the occurrence of one or other of these two "substitutions," either of which affords an adequate and sufficient cause of the metamorphosis.

As the expression $A(x-a)(y-b)$ is the symbol of the event of the transformation of Axy and Aab into Aay and Abx, so the expression $-A(x-a)(y-b)$ is the symbol of the transformation of Aay and Abx into Axy and Aab. This event will be termed, in relation to the former event, the "reverse" of that event or the "reverse event;" and while the symbol $A(x-a)(y-b)$ is interpreted as the symbol of a "substantive" event in which

A is constant, and which occurs by the two substitutions indicated; the symbo $-A(x-a)(y-b)$ is to be interpreted as the symbol of an event which is the "reverse" of an event in which A is constant, and which occurs by those two substitutions,—that is to say, in the latter case the event is defined by specifying the relation in which that event stands to another event, the conception of which is essential to its comprehension. This interpretation is a consequence of the identity (Part I. Sec. II. (4))

$$-A(x-a)(y-b)=+[-A(x-a)(y-b)].$$

Since

$$A(x-a)(y-b)=-A(a-x)(y-b),$$

every event of this kind may be considered from these two points of view, namely, either as a "substantive" event or as the reverse of some other event; and wherever in this Calculus the symbol of a chemical event occurs preceded by the negative sign, thus, $-U$, that symbol is always to be interpreted as though it were written $+(-U)$, namely, as an event which is the reverse of the event U.

In the light of the preceding observations the reader will interpret the following identities, which are demonstrated from the principles established, I. Sec. II. (4) (6),

$$A(x-a)(y-b)=A(a-x)(b-y),$$
$$-A(x-a)(y-b)=A(a-x)(y-b),$$
$$-[-A(x-a)(y-b)]=A(x-a)(y-b).$$

In the previous reasoning the three properties of chemical symbols are utilized which were demonstrated in Part I. Sec. II., namely, that $xy+ab=ab+xy$, that $xy=yx$, and that $A(x\pm a)=Ax\pm Aa$. If any one of these properties should not belong to such symbols, the reasoning altogether fails.

For the application to a chemical equation of the preceding principles, the equation must be expressed by means of some specified set of prime factors. Now the only expression of this kind which we have as yet had occasion to consider is the expression of chemical equations by means of the prime factors ascertained in Part I., namely, the prime factors α, χ, ξ, θ, . . ., which are the symbols of those "weights" which are undistributed in the total system of chemical phenomena. The term "simple" or undistributed weight, however, is a purely relative term, having been defined as a "weight" which in some special system of chemical events is not resolved into two or more weights or made up from such weights (Part I. Sec. I. 12). Hence an equation may be expressed, possibly even in numerous ways, by means of different sets of prime factors according to the system of events to which the event under consideration is referred—these prime factors being indeed the symbols of the "simple weights" among which the "substitutions" are conceived of as occurring which are the causes of the events, every such expression necessarily involves an assertion or an hypothesis as to the composition of some one or more among the units of ponderable matter, of which the transformations are considered, which constitutes the base of the symbolic system, analogous to the fundamental hypothesis which is the base of the system constructed in Part I., namely, that the unit of hydrogen is itself a simple weight and expressed by a single letter α. This is not the place for the further discussion of this question, which I shall hereafter have occasion to consider in some detail; but I have made the preceding remarks to prevent any misapprehension arising in the mind of the reader in reference to the examples of chemical events given in this and the following Section, in which the equations are expressed by various sets of prime factors. These are not, as a matter of fact, mere arbitrary expressions (although, indeed, such would have sufficiently served the object in view), but I have not entered on the reasons which justify them, as being beside my purpose. I need only further observe that the equations are not to be considered collectively as representing systems of events (although they may be selected from such systems), but as individual equations, where a system is reduced to its simplest form, namely to a single event which from some unexplained reason is represented in the way given.

(7) The following event is an example of this class of phenomena. A unit of chloride of benzoyl and a unit of potassium alcohol are identical with a unit of chloride of potassium and a unit of benzoic ether,

$$\alpha^3\kappa^7\xi\chi+\alpha^3\kappa^2\xi\mu=\alpha\chi\mu+\alpha^5\kappa^9\xi^2.$$

This equation may be expressed by means of the prime factors α, χ, μ, $(\alpha^2\kappa^7\xi)$, $(\alpha^2\kappa^2\xi)$, and thus written

$$\alpha(\alpha^2\kappa^7\xi)\chi+\alpha(\alpha^2\kappa^2\xi)\mu=\alpha\chi\mu+\alpha(\alpha^2\kappa^7\xi)(\alpha^2\kappa^2\xi),$$

whence

$$\alpha(\alpha^2\kappa^7\xi-\mu)(\chi-\alpha^2\kappa^2\xi)=0.$$

By which equation we are informed that α in this event is "constant" and that the event occurs in one of two ways, namely, either by the exchange of the "weight" $\alpha^2\kappa^7\xi$ for μ or of χ for $\alpha^2\kappa^2\xi$. It is to be borne in mind that these hypotheses are purely relative to this equation alone, and involve the assumption that in this event the "weights" α, $\alpha^2\kappa^7\xi$, $\alpha^2\kappa^2\xi$, μ, χ are not distributed. If this restriction be removed, other views may be taken of the nature of this occurrence.

(8) It is desirable to notice certain special forms of this fundamental phenomenon. If $a=b$ so that the "values" of y and x become identical, the equation becomes

$$A(x-a)(y-a)=0,$$

and

$$Axy+Aa^2=Aay+Aax.$$

Such an event, for example, is the following:—

Example:—A unit of acetate of chlorine and a unit of iodine are identical with a unit of acetate of iodine and a unit of the chloride of iodine; thus

$$\alpha^2\kappa^2\xi^2\chi+\alpha\omega^2=\alpha^2\kappa^2\xi^2\omega+\alpha\omega\chi,$$

which may be written thus

$$\alpha(\alpha\kappa^2\xi^2)\chi+\alpha\omega^2=\alpha(\alpha\kappa^2\xi^2)\omega\chi+\alpha\omega\chi,$$

whence

$$\alpha(\alpha\kappa^2\xi^2-\omega)(\chi-\omega)=0.$$

In this event, therefore, α is necessarily to be regarded as "constant," and the event is to be referred to one of two "causes," namely, the exchange of $\alpha\kappa^2\xi^2$ for ω or of χ for ω.

If also $x=y$, the equation becomes

$$A(x-a)^2=0,$$

the two causes of the event being identical, and

$$Ax^2+Aa^2=2Aax.$$

Such an event is the following:—

Example:—A unit of chlorine and a unit of iodine are identical with two units of chloride of iodine; thus

$$\alpha\chi^2+\alpha\omega^2=2\alpha\chi\omega,$$

whence

$$\alpha(\chi-\omega)^2=0.$$

In this event α is "constant;" the event occurs by the substitution of ω for χ, and occurs in two ways by that substitution, as is evident on inspecting the equation.

(9) If in the above equation $a=1$, so that the equation becomes

$$A(x-1)(y-b)=0,$$

one of the "causes" of the event is the substitution of 1 for x. Now the symbol 1, being interpreted as the symbol of the "weight" or matter which occupies an empty unit of space, is the symbol of "no weight;" and this "cause," therefore, is the substitution of "no weight" for the weight which results from the performance of the operation x. A substitution of this kind will be termed "a transference." Thus we should say that the above event occurred either by the "substitution" of b for y, or by the "transference" of x.

In introducing this term it is necessary to guard against the supposition that "a transference" is here regarded as a new and peculiar phenomenon distinct from a "substitution." This is not the case; every "transference" is a "substitution," although every substitution is not a "transference." This distinction is, however, really inherent in the received theory of "substitution" or "double decomposition," as explained even by the most competent chemists. Thus KEKULÉ indicates as a blot on the theory of "double decomposition," that this theory is inapplicable to cases of "direct addition," or applicable only with the greatest violence "nicht (oder nur höchst gezwungen) anwendbar" [KEKULÉ, vol. i. p. 142]. In this Calculus the distinction is not abolished, but simply does not appear at all, every chemical event being necessarily referred in it (without any forcing) to the operation of one and the same law of "substitution," a generalization due to the introduction of the chemical symbol 1. The meaning of a remark made in the first part of this Calculus (I. Sec. III. (3)) and there left unexplained, to the effect that the matter of an empty unit of space, although not an object to be presented to the imagination, must nevertheless, if we would reason correctly, be "treated as a reality in the order of ideas," will now be apparent.

Example :—The following event is of the above form,

$$\alpha^3\chi^2\omega+\alpha^2\nu=\alpha\omega+\alpha^4\chi^2\nu,$$

which equation may be written thus

$$\alpha(\omega-\alpha\nu)(\alpha^2\chi^2-1)=0.$$

The event here symbolized is the transformation of a unit of iodide of ethyl and a unit of ammonia into a unit of hydriodic acid and a unit of ethylamine, which event may be referred to one of two causes, namely, either to the substitution of $\alpha\nu$ for ω, or to the transference of $\alpha^2\chi^2$, as is seen on inspecting the above equation.

Example :—Again,

$$\alpha\xi^2+\alpha=2\alpha\xi,$$

or

$$\alpha(\xi-1)^2=0.$$

This equation expresses the event of the transformation of a unit of hydrogen and a unit of binoxide of hydrogen into two units of water.

This event occurs by "the transference" of ξ, and in two ways by that "transference."

Example :—Again take the equation

$$\alpha^5\chi^4\xi+\alpha\xi=2\alpha^3\chi^2\xi,$$

as expressed by the prime factors α, ξ, $(\alpha^2\chi^2)$,

$$\alpha\xi(\alpha^2\chi^2)^2+\alpha\xi=2\alpha\xi(\alpha^2\chi^2),$$

or

$$\alpha\xi(\alpha^2\chi^2-1)^2=0,$$

which symbolizes the event of the transformation of a unit of ether and a unit of water into two units of alcohol.

In this event $\alpha\xi$ (the unit of water) is necessarily to be regarded as constant. The event occurs by "the transference" of $\alpha^2\chi^2$, and in two ways by that "transference."

If in the equation $A(x-a)(y-b)=0$, $A=1$, so that

$$(x-a)(y-b)=0,$$

"no weight" is constant, the event being fully defined when we say that the event occurs by the substitution of a for x or of b for y.

Example :—Such an event, for example, is the transformation of a unit of hydrogen and a unit of sulphide of mercury into a unit of mercury and a unit of sulphide of hydrogen; thus

$$\delta\theta+\alpha=\alpha\theta+\delta,$$

$$(\delta-\alpha)(\theta-1)=0.$$

In which event "no weight" is constant, and the event occurs either by the "substitution" of α for δ or by the transference of θ.

Example:—Or, again, the transformation of a unit of binoxide of hydrogen into a unit of hydrogen and a unit of oxygen, which may be thus expressed,

$$(\alpha-1)((\xi^2)-1)=0.$$

In which event, again, "no weight" is constant, and the event occurs either by the transference of α or the transference of ξ^2, as is apparent from the equation

$$1+\alpha(\xi^2)=\alpha+(\xi^2).$$

(10) The consideration of an event, not as a "substantive" event, but as "the reverse" of some other event, is a principle of very real utility, both as regards our appreciation of the real "causes" of events, and also as regards our appreciation of the analogies and relations of events.

Take, for example, the event of the transformation of a unit of binoxide of hydrogen and a unit of sulphurous acid into a unit of sulphuric acid, the result of which is given in the equation

$$\alpha\xi^2+\theta\xi^2=1+\alpha\theta\xi^4.$$

Now this equation may be expressed by the prime factors $(\alpha\xi^2)$ and $(\theta\xi^2)$ and written thus,

$$(\alpha\xi^2)+(\theta\xi^2)=1+(\alpha\xi^2)(\theta\xi^2).$$

The symbol of this occurrence, regarded as a substantive event, is

$$(1-\alpha\xi^2)(\theta\xi^2-1)=0,$$

which compels us to interpret the factor $1-(\alpha\xi^2)$, in which case it would be necessary to introduce the symbol 1 into the equation in an explicit form. But these difficulties disappear when we write the equation thus,

$$+[-(\alpha\xi^2-1)(\theta\xi^2-1)]=0.$$

In this case we do not define the above event as a substantive event; but we simply say that it is the reverse of another event, the nature of which we can readily comprehend, the two alternative causes of which are the transference of $(\alpha\xi^2)$ and the transference of $(\theta\xi^2)$.

Again, the equation

$$\alpha+\alpha\chi^2=2\alpha\chi,$$

which expresses the formation of hydrochloric acid, may be written thus,

$$\alpha(\chi-1)^2=0,$$

being an event in which α is constant, and which occurs in two ways by the transference of χ.

Now the resolution of hydrochloric acid into its elements is thus expressed,

$$2\alpha\chi=\alpha+\alpha\chi^2,$$

the symbol of which, regarded as a substantive event, is

$$\alpha(1-\chi)(\chi-1)=0.$$

Instead of attempting to interpret the factor $(1-\chi)$ we may write this event thus,

$$+[-\alpha(\chi-1)^2]=0;$$

in which case we do not attempt to explain the "causes" of the event itself, but instead of so doing, we say that the event is the reverse of an event of which both causes are intelligible to us.

The events which we have now considered are completely defined by their symbolical expression. They are events which may be referred to two alternative "causes," and to two such "causes" alone; that is to say, they are events which necessarily occur by one or the other of two specified substitutions, and by this property are, as a class, separated from all other phenomena. This great class of events is, however, only one, and that the very simplest, instance of an indefinite number of such systems to the conception and consideration of which we are brought by the development of the methods of this Calculus, and to the explanation of which I shall now proceed. Of these systems, also, I shall lay before the reader a sufficient number of examples to invest these conceptions with reality, and to satisfy him that we are not dealing simply with algebraical forms, but with algebraical forms corresponding to real occurrences. It will not, however, be necessary to dwell at equal length upon this portion of the subject; for the reader who has accompanied me so far will readily appreciate what I have now to submit to him.

(11) If an equation be expressed as the continued product of three factors each of the form $x-a$, so that

$$A(x-a)(y-b)(z-c)=0,$$

the event may be conceived of as occurring by one substitution and, as thus occurring, in three different ways, namely, by the substitution of a for x and of b for y and of c for z, which three "substitutions" are to be regarded as three alternative "causes" of that event, to the one or the other of which that event must necessarily be referred. For the result of this event is expressed in the equation

$$Axyz+Aabz+Aayc+Axbc=Axyc+Aabc+Aayz+Axbz;$$

and if, proceeding as before, we institute a comparison between the arrangement before the event and the arrangement after the event, with the view of ascertaining experimentally the substitutions which are the causes of the event, superposing the one arrangement upon the other, thus—

$$\mathrm{A}xyz+\mathrm{A}abz+\mathrm{A}ayc+\mathrm{A}xbc, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{I.}$$

$$\mathrm{A}xyc+\mathrm{A}abc+\mathrm{A}ayz+\mathrm{A}xbz, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{II.}$$

it is apparent that where z appears in the arrangement (I.) before the event c appears in the arrangement (II.) after the event, and where c appears in the arrangement (I.) before the event z appears in the arrangement (II.) after the event, and the arrangements differ in that respect, and in that respect alone. That is to say, according to the definition, Section III. (1), the event occurs by the substitution of c for z. But since

$$\mathrm{A}(x-a)(y-b)(z-c)=\mathrm{A}(x-a)(z-c)(y-b),$$

the above reasoning in regard to z and c is equally correct with regard to y and b, that is to say, the event also occurs by the "substitution" of b for y; and similarly the event also occurs by the "substitution" of a for x; and there are no other two letters in regard to which a similar assertion can be made.

(12) Every actual example of such an event in which x, y, z and a, b, c are all different involves the construction of no less than eight different chemical substances connected by the relation of identity expressed in the above equation. The problem of the realization of such an event is greatly simplified where two or more among the letters x, y, z or a, b, c become identical. The simplest case, from this point of view (the case, that is, which involves the construction of the smallest number of different substances), is that in which $x=y=z$, and also $a=b=c$. The equation then becomes

$$\mathrm{A}(x-a)^3=0,$$

and

$$\mathrm{A}x^3+3\mathrm{A}a^2x=3\mathrm{A}ax^2+\mathrm{A}a^3.$$

Example:—A unit of chloroform and three units of chloride of methyl are identical with three units of mono-methyl chloride and a unit of marsh-gas; thus

$$(\alpha^2\kappa)\chi^3+3(\alpha^2\kappa)\chi=3(\alpha^2\kappa)\chi^2+(\alpha^2\kappa),$$

whence

$$\alpha^2\kappa(\chi-1)^3=0.$$

The event therefore is fully defined by saying that the above phenomenon is an event in which $\alpha^2\kappa$ (the symbol of the unit of marsh-gas) is "constant," and which occurs by the transference of χ, and occurs by that transference in three ways.

Example:—A unit of the trichlorhydrine of glycerine and three units of monochlorhydrine are identical with a unit of glycerine and three units of dichlorhydrine; thus

$$\alpha^4\kappa^3\chi^3+3\alpha^4\kappa^3\xi^2\chi=\alpha^4\kappa^3\xi^3+3\alpha^4\kappa^3\xi\chi^2,$$

whence

$$\alpha^4\kappa^3(\chi-\xi)^3=0.$$

In this event $\alpha^4\kappa^3$, the unit of the (so called) hydride of propyl, is constant. The event occurs by the substitution of ξ for χ, and by that substitution in three ways.

Example:—A unit of glycerine and three units of diacetine are identical with three units of monacetine and a unit of triacetine,

$$\alpha^4\kappa^3\xi^3+3\alpha^6\kappa^7\xi^5=3\alpha^5\kappa^5\xi^4+\alpha^7\kappa^9\xi^6,$$

which equation may be written thus,

$$\alpha^4\kappa^3\xi^3+3\alpha^4\kappa^3\xi^3(\alpha\kappa^2\xi)^2=3\alpha^4\kappa^3\xi^3(\alpha\kappa^2\xi)+\alpha^4\kappa^3\xi^3(\alpha\kappa^2\xi)^3,$$

whence

$$-\alpha^4\kappa^3\xi^3(\alpha\kappa^2\xi-1)^3.$$

That is to say, this event is the reverse of an event in which $\alpha^4\kappa^3\xi^3$ (the unit of glycerine) is constant, and which occurs by the transference of $\alpha\kappa^2\xi$, and in three ways by that transference.

If in the above equation $x=y$ and $a=b=c$, the equation becomes

$$A(x-a)^3(z-a)=0,$$

and

$$Ax^3z+2Aa^3x+Aa^3z=Aax^3+2Aaxz+Aa^3.$$

Example:—A unit of ethyldiacetamide and two units of acetamide and a unit of ethylamine are identical with a unit of diacetamide and two units of diethylmonacetamide and a unit of ammonia [ref. KEKULÉ, vol. i. p. 575]; thus

$$\alpha^6\kappa^6\xi^2\nu+2\alpha^3\kappa^2\xi\nu+\alpha^4\kappa^2\nu=\alpha^4\kappa^4\xi^2\nu+2\alpha^6\kappa^4\xi\nu+\alpha^2\nu,$$

which equation may be put in the following form,

$$\alpha^2\nu(\alpha\kappa^2\xi)^2(\alpha^2\kappa^2)+2\alpha^2\nu(\alpha\kappa^2\xi)+\alpha^2\nu(\alpha^2\kappa^2)=\alpha^2\nu(\alpha\kappa^2\xi)^2+2\alpha^2\nu(\alpha\kappa^2\xi)(\alpha^2\kappa^2)^2+\alpha^2\nu,$$

whence

$$\alpha^2\nu(\alpha\kappa^2\xi-1)^2(\alpha^2\kappa^2-1)=0.$$

In this event $\alpha^2\nu$ (the unit of ammonia) is constant, and the event occurs by the transference of $\alpha^2\kappa^2$ and by the transference of $\alpha\kappa^2\xi$, and by the latter transference in two ways.

Example:—A unit of diethylamylamine, a unit of amylamine, and two units of ethylamine are identical with a unit of ammonia, a unit of diethylamine, and two units of ethylamylamine,

$$\alpha^{11}\kappa^9\nu+\alpha^7\kappa^5\nu+2\alpha^4\kappa^2\nu=\alpha^2\nu+\alpha^6\kappa^4\nu+2\alpha^9\kappa^7\nu.$$

This equation may be written thus,

$$\alpha^2\nu(\alpha^2\kappa^2)^2(\alpha^5\kappa^5)+\alpha^2\nu(\alpha^5\kappa^5)+2\alpha^2\nu(\alpha^2\kappa^2)=\alpha^2\nu+\alpha^2\nu(\alpha^2\kappa^2)^2+2\alpha^2\nu(\alpha^2\kappa^2)(\alpha^5\kappa^5),$$

whence

$$\alpha^2\nu(\alpha^2\kappa^2-1)^2(\alpha^5\kappa^5-1)=0.$$

In this event $\alpha^2\nu$ (the unit of ammonia) is constant. The event occurs either by the transference of $\alpha^5\kappa^5$ or by the transference of $\alpha^2\kappa^2$, and by the latter transference in one of two ways.

If $a=b=c$, the equation becomes

$$A(x-a)(y-a)(z-a)=0,$$

and

$$Axyz+Aa^2x+Aa^2y+Aa^2z=Aaxy+Aaxz+Aayz+Aa^3.$$

Example:—A unit of methylethylamylamine and a unit of methylamine and a unit of ethylamine and a unit of amylamine are identical with a unit of methylethylamine and a unit of methylamylamine and a unit of ethylamylamine and a unit of ammonia; thus

$$\alpha^{10}\kappa^8\nu+\alpha^3\kappa\nu+\alpha^4\kappa^2\nu+\alpha^7\kappa^5\nu=\alpha^5\kappa^3\nu+\alpha^8\kappa^6\nu+\alpha^9\kappa^7\nu+\alpha^2\nu,$$

which equation may be written thus,

$$(\alpha^2\nu)(\alpha\kappa)(\alpha^2\kappa^2)(\alpha^5\kappa^5)+(\alpha^2\nu)(\alpha\kappa)+(\alpha^2\nu)(\alpha^2\kappa^2)+(\alpha^2\nu)(\alpha^5\kappa^5)=(\alpha^2\nu)(\alpha\kappa)(\alpha^2\kappa^2)$$
$$+(\alpha^2\nu)(\alpha\kappa)(\alpha^5\kappa^5)+(\alpha^2\nu)(\alpha^2\kappa^2)(\alpha^5\kappa^5)+(\alpha^2\nu),$$

whence

$$\alpha^2\nu(\alpha\kappa-1)(\alpha^2\kappa^2-1)(\alpha^5\kappa^5-1)=0;$$

$\alpha^2\nu$ (the unit of ammonia) is constant, and the event occurs by the transference of $\alpha\kappa$, and by the transference of $\alpha^2\kappa^2$, and by the transference of $\alpha^5\kappa^5$.

If $a=b$ the equation becomes

$$A(x-a)(y-a)(z-c)=0,$$

and

$$Axyz+Aacx+Aacy+Aa^2z=Acxy+Aaxz+Aayz+Aa^2c.$$

Example:—A unit of the acetobromochlorhydrine of glycerine and a unit of monacetine and a unit of the monobromohydrine of glycerine and a unit of the monochlorhydrine of glycerine are identical with a unit of glycerine and a unit of the bromochlorhydrine of glycerine and a unit of the acetobromohydrine of glycerine and a unit of the acetochlorhydrine of glycerine, whence

$$\alpha^5\kappa^5\xi^2\beta\chi+\alpha^5\kappa^5\xi^4+\alpha^4\kappa^3\xi^2\beta+\alpha^4\kappa^3\xi^2\chi=\alpha^4\kappa^3\xi^3+\alpha^4\kappa^3\xi\beta+\alpha^5\kappa^5\xi^3\beta+\alpha^5\kappa^5\xi^3\chi.$$

This equation may be written thus,

$$(\alpha^4\kappa^3\xi)(\alpha\kappa^2\xi)\beta\chi+(\alpha^4\kappa^3\xi)(\alpha\kappa^2\xi)\xi^2+(\alpha^4\kappa^3\xi)\beta\xi+(\alpha^4\kappa^3\xi)\xi\chi=(\alpha^4\kappa^3\xi)\xi^2$$
$$+(\alpha^4\kappa^3\xi)\beta\chi+(\alpha^4\kappa^3\xi)(\alpha\kappa^2\xi)\beta\xi+(\alpha^4\kappa^3\xi)(\alpha\kappa^2\xi)\xi\chi,$$

whence

$$\alpha^4\kappa^3\xi(\beta-\xi)(\chi-\xi)(\alpha\kappa^2\xi-1)=0.$$

We are thus informed that in this event $\alpha^4\kappa^3\xi$ is constant, and that there are three causes of the event, namely, the "transference" of $\alpha\kappa^2\xi$, the substitution of ξ for χ, and the substitution of ξ for β.

(13) By reasoning analogous to that previously employed, it may be readily demonstrated that if a chemical equation be expressed by four factors of the form $x-a$, so that

$$A(x-a)(y-b)(z-c)(v-d)=0,$$

the event symbolized in that equation may occur by one substitution, and may thus occur in four different ways; that is to say, there are four alternative "causes" of that event, to one or the other of which the event must necessarily be referred, namely, the substitution of a for x, of b for y, of c for z, and of d for v. The same principle may be extended to any number of factors; and generally, if a chemical equation be expressed by n such factors, so that

$$A(x-a)(y-b)(z-c)(v-d)(w-e)\ \ldots\ldots=0,$$

the event symbolized in that equation may occur by one substitution, and may thus occur in n ways; that is to say, there are n alternative "causes" of that event to one or other of which the event must necessarily be referred, namely, the n substitutions indicated by the n factors of that equation.

As the complexity of chemical phenomena increases so do these phenomena, regarded as actual events, become more rare, and it is more and more difficult to give real examples of them. This arises mainly perhaps from the difficulty inherent in the way of their experimental realization, an event expressed even by four different factors requiring the construction of no less than sixteen different substances connected by a certain definite relation, but also from the circumstance that the efforts of chemists have never been directed to the realization of such phenomena as a definite problem. We have the strongest evidence in favour of the existence of these relations, but we cannot exhibit them in a complete form. Such events are, if we may so say, fragmentary and imperfect, like buildings in process of construction, some near to completion, others of which the plan and outline are visible to the eye of the architect alone.

The following events are examples of this class:—

Example:—A unit of tetrachloride of carbon together with six units of monomethyl chloride and a unit of marsh-gas are identical with four units of chloroform and four units of chloride of methyl; thus

$$(\alpha^2\kappa)\chi^4+6(\alpha^2\kappa)\chi^2+(\alpha^2\kappa)=4(\alpha^2\kappa)\chi^3+4(\alpha^2\kappa)\chi,$$

whence

$$(\alpha^2\kappa)(\chi-1)^4=0.$$

In this event $\alpha^2\kappa$, the symbol of the unit of marsh-gas, is constant. The event occurs by the transference of χ, and in four ways by that transference.

Example:—A unit of tetrachloride of tin, a unit of stannic diethylchloride, four units of stannic ethylmethylchloride [unknown], a unit of stannic dimethylchloride, and a unit of stannic diethyldimethyl are identical with two units of stannic methyltrichloride, two units of stannic methylchloridediethyl [unknown], two units of stannic ethylchloridedimethyl, and two units of stannic ethyltrichloride*, whence

$$\alpha^2\kappa'\chi^4+\alpha^6\kappa'\kappa^4\chi^2+4\alpha^5\kappa'\kappa^3\chi^2+\alpha^4\kappa'\kappa^2\chi^2+\alpha^8\kappa'\kappa^6=2\alpha^3\kappa'\kappa\chi^3+2\alpha^7\kappa'\kappa^5\chi+2\alpha^6\kappa'\kappa^4\chi+2\alpha^4\kappa'\kappa^2\chi^3.$$

This equation may be written thus,

$$(\alpha^2\kappa')\chi^4+(\alpha^2\kappa')(\alpha^2\kappa^2)^2\chi^2+4(\alpha^2\kappa')(\alpha\kappa)(\alpha^2\kappa^2)\chi^2+(\alpha^2\kappa')(\alpha\kappa)^2\chi^2+(\alpha^2\kappa')(\alpha\kappa)^2(\alpha^2\kappa^2)^2$$
$$=2(\alpha^2\kappa')(\alpha\kappa)\chi^3+2(\alpha^2\kappa')(\alpha\kappa)(\alpha^2\kappa^2)^2\chi+2(\alpha^2\kappa')(\alpha\kappa)^2(\alpha^2\kappa^2)\chi+2(\alpha^2\kappa')(\alpha^2\kappa^2)\chi^3,$$

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* *Vide* KEKULÉ, vol. i. 782, p. 505.

whence

$$(\alpha^2\kappa)(\chi-\alpha\kappa)^2(\chi-\alpha^2\kappa^2)^2=0.$$

This event, therefore, is to be considered as occurring by the substitution of $\alpha\kappa$ for χ and by the substitution of $\alpha^2\kappa^2$ for χ, and in two ways by each of these substitutions respectively.

SECTION IV.

We thus arrive at the interpretation of the chemical equation

$$A(x-a)(y-b)(z-c)\ldots=0,$$

considered as the symbol of a "simple chemical event." From the consideration of such an equation we are led to refer the event thus symbolized to certain specified substitutions (indicated to us by the factors of the equation) as the causes of that event, by the enumeration of which causes, together with the constant A, the event is defined, there being only one possible event which will satisfy the conditions specified in the equation. Such equations constitute a class apart. The explanation of a chemical event consists in referring the event to the causes which concur to produce it. Therefore it is only those events which can be thus expressed which can, in any proper sense, be said to be explained, simple events, and such events admit of no further explanation of this kind.

Now a chemical equation may not be of the above form, an equation to a "simple event," but yet may be expressed by means of rational factors. In this case the event indicated in the equation may be immediately resolved into some number of simple events occurring by one or more specified substitutions, of which that event is the aggregate. Such aggregates were the first actually discovered by me; and although in the next section the whole question of the analysis of aggregates will be discussed in a far more complete manner, I shall yet invite the reader, with a view to a clear comprehension of the subject, to follow the course actually pursued, and to consider a few of the simpler problems which such equations present to us.

Examples.

(1) A unit of chlorine and two units of hydriodic acid are identical with a unit of iodine and two units of hydrochloric acid, and therefore may be transformed into these units.

The equation to this event is

$$\alpha\chi^2+2\alpha\omega=2\alpha\chi+\alpha\omega^2,$$

which equation may be thus expressed,

$$\alpha(\chi+\omega-2)(\chi-\omega)=0.$$

Now we cannot interpret the equation in this form; for although, it is true, we know what is meant by the factor $(\chi-\omega)$ and can perform the substitution which it directs us to perform, the factor $(\chi+\omega-2)$, which should express the alternative mode of the occurrence of the event, conveys no meaning to us. We can, however, easily convert the equation into an interpretable form by resolving this factor. If this be effected the equation to the event becomes

$$\alpha(\chi-1)(\chi-\omega)+\alpha(\omega-1)(\chi-\omega)=0.$$

From the principles demonstrated in Section I. (4) each of the terms of this equation may be equated to zero, and we are thus informed that this event is an aggregate of the two simple events,

$$\alpha(\chi-1)(\chi-\omega)=0,$$
$$\alpha(\omega-1)(\chi-\omega)=0.$$

Since these equations have a factor in common $(\chi-\omega)$, the events expressed in them may be referred to a common cause, namely, the substitution of ω for χ, and may be regarded as members of a system of two events (in which α is constant) occurring by this substitution. At the same time we have in each case an alternative cause of the event, namely, in the former of the two events the transference of χ, in the latter the transference of ω, the original event regarded in its result being

$$\alpha\chi^2+2\alpha\omega=2\alpha\chi+\alpha\omega^2,$$

and the constituents of that event similarly regarded being

$$\alpha\chi^2+\alpha\omega=\alpha\chi+\alpha\omega\chi,$$
$$\alpha\omega\chi+\alpha\omega=\alpha\omega^2+\alpha\chi.$$

The synthesis here indicated has been actually effected, the unit $\alpha\omega\chi$ being no other than the unit of the chloride of iodine, which is formed, together with a unit of hydrochloric acid, by the action of a unit of chlorine on a unit of hydriodic acid according to the former equation, and a unit of which, together with a unit of hydriodic acid, is resolved into a unit of iodine and a unit of hydrochloric acid according to the latter equation.

(2) A unit of chlorosulphuric acid and two units of water are identical with a unit of sulphuric acid and two units of hydrochloric acid, thus

$$\alpha\theta\xi^2\chi^2+2\alpha\xi=\alpha\theta\xi^4+2\alpha\chi.$$

This equation may be thus expressed,

$$\alpha(\theta\xi^2\chi+\theta\xi^3-2)(\chi-\xi)=0.$$

Here, again, we cannot interpret one of the factors of the equation, namely $(\theta\xi^2\chi+\theta\xi^3-2)$; but proceeding as before we may resolve this factor, and bring the equation into an interpretable form, thus

$$\alpha(\theta\xi^2\chi-1)(\chi-\xi)+\alpha(\theta\xi^3-1)(\chi-\xi)=0.$$

Equating as before each term of this equation to zero, we are informed that the event is an aggregate of the two events

$$\alpha(\theta\xi^2\chi-1)(\chi-\xi)=0,$$
$$\alpha(\theta\xi^3-1)(\chi-\xi)=0.$$

In this case, again, the two events may be referred to a common cause, namely the substitution of ξ for χ. But in each case an alternative is presented to us, namely, in the former of the two events the transference of $(\theta\xi^2\chi)$, regarded as the symbol of a simple weight, and one of the prime factors of the equation, and in the latter event the transference of $(\theta\xi^3)$ similarly regarded.

The event

$$\alpha\theta\xi^2\chi^2+2\alpha\xi=\alpha\theta\xi^4+2\alpha\chi$$

is thus resolved into the constituents

$$\alpha\theta\xi^2\chi^2+\alpha\xi=\alpha\theta\xi^3\chi+\alpha\chi,$$
$$\alpha\theta\xi^3\chi+\alpha\xi=\alpha\theta\xi^4+\alpha\chi,$$

both of which events may be considered to occur by the substitution of ξ for χ.

The synthesis of this phenomenon here suggested to us has actually been realized—the unit of matter, $\alpha\theta\xi^3\chi$, which is thus brought under our notice being the unit of the substance termed hydrochlorosulphurous acid, which is formed, together with other products, by the action of pentachloride of phosphorus on sulphuric acid, and is to be recognized as identical with the substance the unit of which appears in these equations by the property which it possesses of being resolved, together with a unit of water, into a unit of hydrochloric acid and a unit of sulphuric acid.

(3) A unit of bromide of ethylene and two units of water are resoluble into two units of hydrobromic acid and a unit of glycol, thus

$$\alpha^3\kappa^2\beta^2+2\alpha\xi=2\alpha\beta+\alpha^3\kappa^2\xi^2.$$

This equation may be thus expressed

$$\alpha(\alpha^2\kappa^2\beta+\alpha^2\kappa^2\xi-2)(\beta-\xi)=0,$$

whence

$$\alpha(\alpha^2\kappa^2\beta-1)(\beta-\xi)+\alpha(\alpha^2\kappa^2\xi-1)(\beta-\xi)=0,$$

the constituents of the event being

$$\alpha(\alpha^2\kappa^2\beta-1)(\beta-\xi)=0,$$
$$\alpha(\alpha^2\kappa^2\xi-1)(\beta-\xi)=0.$$

In these events α is constant; the two events may be referred to a common cause, namely the substitution of ξ for β. The results are

$$\alpha^3\kappa^2\beta^2+\alpha\xi=\alpha\beta+\alpha^3\kappa^2\beta\xi,$$
$$\alpha^3\kappa^2\beta\xi+\alpha\xi=\alpha\beta+\alpha^3\kappa^2\xi^2.$$

These events have been realized, the unit $\alpha^3\kappa^2\beta\xi$, intermediate in composition between glycol and bromide of ethylene, being the monobromohydrin of glycol.

(4) A unit of diacetic glycol and two units of water are identical with two units of acetic acid and a unit of glycol, thus

$$\alpha^5\kappa^6\xi^4+2\alpha\xi=2\alpha^2\kappa^2\xi^2+\alpha^3\kappa^2\xi^2.$$

This equation may be thus expressed,

$$\alpha\xi(\alpha^2\kappa^2\xi+\alpha^3\kappa^4\xi^2-2)(\alpha\kappa^2\xi-1)=0,$$

whence

$$\alpha\xi(\alpha^2\kappa^2\xi-1)(\alpha\kappa^2\xi-1)+\alpha\xi(\alpha^3\kappa^4\xi^2-1)(\alpha\kappa^2\xi-1)=0,$$

the constituents of the event being

$$\alpha\xi(\alpha^2\kappa^2\xi-1)(\alpha\kappa^2\xi-1)=0,$$
$$\alpha\xi(\alpha^3\kappa^4\xi^2-1)(\alpha\kappa^2\xi-1)=0.$$

In these events $\alpha\xi$ is constant; the two events may be referred to a common cause, namely the transference of $\alpha\kappa^2\xi$. The resulting identities are

$$\alpha^4\kappa^4\xi^3+\alpha\xi=\alpha^3\kappa^2\xi^2+\alpha^2\kappa^2\xi^2,$$
$$\alpha^5\kappa^6\xi^4+\alpha\xi=\alpha^4\kappa^4\xi^3+\alpha^2\kappa^2\xi^2.$$

These phenomena have been realized, the unit $\alpha^4\kappa^4\xi^3$, which together with a unit of acetic acid is formed by the decomposition of a unit of diacetic glycol and a unit of water, is the unit of monacetic glycol, which has been prepared by the simultaneous decomposition of a unit of bromide of ethylene, a unit of acetate of potash, and a unit of water.

(5) A unit of suberic acid is identical with a unit of the hydride of hexyl and two units of carbonic acid; this relation is given in the equation

$$2+\alpha^7\kappa^8\xi^4=\alpha^7\kappa^6+2\kappa\xi^2.$$

This equation may be thus expressed

$$(\alpha^7\kappa^7\xi^2+\alpha^7\kappa^6-2)(\kappa\xi^2-1)=0,$$

whence

$$(\alpha^7\kappa^7\xi^2-1)(\kappa\xi^2-1)+(\alpha^7\kappa^6-1)(\kappa\xi^2-1)=0,$$

the constituents of the event being

$$(\alpha^7\kappa^7\xi^2-1)(\kappa\xi^2-1)=0,$$
$$(\alpha^7\kappa^6-1)(\kappa\xi^2-1)=0,$$

which two events may be referred to a common cause, namely the transference of $\kappa\xi^2$, the results being

$$\alpha^7\kappa^8\xi^4+1=\alpha^7\kappa^7\xi^2+\kappa\xi^2,$$
$$\alpha^7\kappa^7\xi^2+1=\alpha^7\kappa^6+\kappa\xi^2.$$

The unit $\alpha^7\kappa^7\xi^2$, which appears in these events, but not in the original event, is the unit of œnanthylic acid, the existence of which unit and the properties by which it may be recognized are here pointed out.

(6) A unit of terchloride of phosphorus and three units of water are resoluble into a unit of hydrated phosphorous acid and three units of hydrochloric acid, thus

$$\alpha^3\phi\chi^3+3\alpha\xi=3\alpha\chi+\alpha^3\phi\xi^3.$$

This equation vanishes when $\chi=\xi$, and may be thus expressed

$$\alpha(\alpha\phi\chi^2+\alpha\phi\chi\xi+\alpha\phi\xi^2-3)(\chi-\xi)=0,$$

whence the event is constituted as follows—

$$\alpha(\alpha\phi\chi^2-1)(\chi-\xi)+\alpha(\alpha\phi\chi\xi-1)(\chi-\xi)+\alpha(\alpha\phi\xi^2-1)(\chi-\xi)=0;$$

which equation may be resolved into the three equations

$$\alpha(\alpha\phi\chi^2-1)(\chi-\xi)=0,$$
$$\alpha(\alpha\phi\chi\xi-1)(\chi-\xi)=0,$$
$$\alpha(\alpha\phi\xi^2-1)(\chi-\xi)=0,$$

the results being

$$\alpha^2\phi\chi^3+\alpha\xi=\alpha^2\phi\chi^2\xi+\alpha\chi,$$
$$\alpha^2\phi\chi^2\xi+\alpha\xi=\alpha^2\phi\chi\xi^2+\alpha\chi,$$
$$\alpha^2\phi\chi\xi^2+\alpha\xi=\alpha^2\phi\xi^3\ +\alpha\chi.$$

No one of these phenomena has hitherto been realized; but we are thus led to anticipate their future realization. The unit $\alpha^2\phi\chi\xi^2$, which is characterized by the property of being, together with a unit of water, resoluble into a unit of phosphorous acid and a unit of hydrochloric acid, is (what may be termed) the unit of the monochlorhydrin of phosphorous acid, the unit $\alpha^2\phi\chi^2\xi$ being the dichlorhydrin of the same substance, the existence of which is also indicated to us by a variety of other considerations.

(7) A unit of the trichlorhydrin of glycerin and three units of sulphide of hydrogen are resoluble into three units of hydrochloric acid and a unit of trithioglycerin—

$$\alpha^4\kappa^3\chi^3+3\alpha\theta=3\alpha\chi+\alpha^4\kappa^3\theta^3.$$

This equation may be thus expressed

$$\alpha(\alpha^3\kappa^3\theta^2+\alpha^3\kappa^3\chi\theta+\alpha^3\kappa^3\chi^2-3)(\chi-\theta)=0,$$

the event being constituted as follows—

$$\alpha(\alpha^3\kappa^3\theta^2-1)(\chi-\theta)+\alpha(\alpha^3\kappa^3\chi\theta-1)(\chi-\theta)+\alpha(\alpha^3\kappa^3\chi^2-1)(\chi-\theta)=0;$$

whence we have as the constituents of the event,

$$\alpha(\alpha^3\kappa^3\theta^2-1)(\chi-\theta)=0,$$
$$\alpha(\alpha^3\kappa^3\chi\theta-1)(\chi-\theta)=0,$$
$$\alpha(\alpha^3\kappa^3\chi^2-1)(\chi-\theta)=0,$$

or

$$\alpha^4\kappa^3\theta^2\chi+\alpha\theta=\alpha\chi+\alpha^4\kappa^3\theta^3,$$
$$\alpha^4\kappa^3\theta\chi^2+\alpha\theta=\alpha\chi+\alpha^4\kappa^3\chi\theta^2,$$
$$\alpha^4\kappa^3\chi^3+\alpha\theta=\alpha\chi+\alpha^4\kappa^3\chi^2\theta.$$

These three equations inform us of the origin and properties of the units of the chlorhydrins of the sulphur-derivatives of glycerin, $\alpha^4x^3\theta\chi^2$, $\alpha^4x^3\theta^2\chi$, which substances have been procured by CARIUS*. In these three events α is constant, and the three events may be referred to one cause, namely the substitution of θ for χ.

(8) A unit of glycerin and three units of acetic acid are resoluble into three units of water and a unit of triacetin, whence

$$\alpha^4x^3\xi^3+3\alpha^2x^2\xi^2=3\alpha\xi+\alpha^7x^9\xi^6.$$

This equation may be thus expressed

$$-\alpha\xi(\alpha^3x^3\xi^2+\alpha^4x^5\xi^3+\alpha^5x^7\xi^4-3)(\alpha x^2\xi-1)=0.$$

Proceeding as in the last example, we arrive at the following constituents of the event:—

$$\alpha^4x^3\xi^3+\alpha^2x^2\xi^2=\alpha\xi+\alpha^5x^5\xi^4,$$
$$\alpha^5x^5\xi^4+\alpha^2x^2\xi^2=\alpha\xi+\alpha^6x^7\xi^5,$$
$$\alpha^6x^7\xi^5+\alpha^2x^2\xi^2=\alpha\xi+\alpha^7x^9\xi^6.$$

In these events $\alpha\xi$, the unit of water, is constant. The three events may be referred to one cause, namely the transference of $\alpha x^2\xi$. These events have been severally realized, the experiments by which BERTHELOT procured the units of the three acetates of glycerin, monacetin $\alpha^5x^5\xi^4$, diacetin $\alpha^6x^7\xi^5$, and triacetin $\alpha^7x^9\xi^6$, being recorded in these three equations.

(9) Three units of iodide of ethyl and a unit of ammonia are identical with three units of hydriodic acid and a unit of triethylamine,

$$3\alpha^3x^2\omega+\alpha^2\nu=3\alpha\omega+\alpha^8x^6\nu.$$

This equation may be thus expressed

$$\alpha(3\omega-\alpha^5x^4\nu-\alpha^3x^2\nu-\alpha\nu)(\alpha^2x^2-1)=0;$$

whence we have the following constituents of this event:—

$$\alpha(\omega-\alpha^5x^4\nu)(\alpha^2x^2-1)=0,$$
$$\alpha(\omega-\alpha^3x^2\nu)(\alpha^2x^2-1)=0,$$
$$\alpha(\omega-\alpha\nu)(\alpha^2x^2-1)=0.$$

In these events α is constant; the three events may be referred to a common cause, namely the transference of α^2x^2. The results of these events are given in the following equations:—

$$\alpha^3x^2\omega+\alpha^6x^4\nu=\alpha\omega+\alpha^8x^6\nu,$$
$$\alpha^3x^2\omega+\alpha^4x^2\nu=\alpha\omega+\alpha^6x^4\nu,$$
$$\alpha^3x^2\omega+\alpha^2\nu\ \ =\alpha\omega+\alpha^4x^2\nu.$$

The three phenomena of which the above event is the aggregate, the realization of which is here suggested to us, are the successive stages by which triethylamine has actually been formed in the investigations of HOFMANN.

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* Annalen der Chemie, cxxiv. 221.

(10) Lastly, take the familiar phenomenon of the decomposition of water into its elements.

Two units of water are identical with two units of hydrogen and a unit of oxygen, thus

$$1+2\alpha\xi=2\alpha+\xi^2.$$

This equation may be thus expressed,

$$(2\alpha-1-\xi)(\xi-1)=0;$$

whence

$$(\alpha-\xi)(\xi-1)+(\alpha-1)(\xi-1)=0,$$

the constituents of the event being

$$(\alpha-\xi)(\xi-1)=0,$$
$$(\alpha-1)(\xi-1)=0.$$

These two events may be referred to a common cause, namely the transference of ξ. Their results are given in the equations

$$\alpha\xi+\xi=\xi^2+\alpha,$$
$$\alpha\xi+1=\xi+\alpha.$$

The element ξ which appears in these phenomena has not been isolated, but its existence is indicated to us as an object of research not here alone, but also by the analyses of numerous other chemical events.

The previous examples are offered to the consideration of the reader, not as illustrations of the general treatment of the subject to be ultimately pursued, but as examples of the most elementary application of the principles of algebraical reasoning, according to the fundamental methods of this calculus, to the analysis of phenomena. By the application to chemical equations of purely formal processes, we elicit from them new and true information as to the chemical occurrences of which they express the results. From this point of view the preceding analyses have a special interest. So far as I am aware they are the very first application which has been made of what can be termed (in any exact sense) algebraical reasoning in chemistry. Such examples might be indefinitely multiplied; but there would be no object in proceeding further in this direction. The method here employed is obviously of but narrow application, being limited to such equations as can be expressed by rational factors (which is an exceptional case), and is superseded by the more general treatment of the subject developed in the following section, to which I will now proceed.

SECTION V.

(1) We have now constructed, in accordance with the method of this calculus, a symbolical representation of simple chemical events, according to which these events are referred to a definite system of causes, and are conceived of as occurring by means of

operations of which the result is the mutual exchange of the simple weights of which the units of matter are constituted. Here, again, if we would reason correctly, it is essential to discriminate between the operation and the result of the operation. The symbol $(x-a)$ occurring among the factors of a chemical equation is an interpretable symbol, which is defined, not by reference to a hypothetical process, but as the symbol of an operation of which the result is the exchange of the bit of matter resulting from the operation a for the bit of matter resulting from the operation x. As, for example, the expression $(\chi-\xi)$ is the symbol of the operation of which the result is the exchange, between two units of matter, of the bit of the matter of oxygen resulting from the operation ξ which weighs 0·715 grm., for the bit of the matter of chlorine resulting from the operation χ which weighs 1·592 grm., the exchange occurring between two portions of matter the nature of which is not specified, but which severally occupy in the gaseous condition at standard temperature and pressure the space of 1000 cubic centims. But this symbol cannot be interpreted as the symbol of a portion of ponderable matter, for there is no reason to believe the simple weight $w(\xi)$ to be contained in the simple weight $w(\chi)$, and there is no external reality, as far as we are aware, corresponding to the difference $w(\chi)-w(\xi)$. Nor, again, are we to imagine that the process by which this result is attained consists in the actual exchange, in their totality, of these bits of matter the one for the other, as we may exchange a white ball for a red ball. On the contrary, when the unit of hydrochloric acid $\alpha\chi$ passes into the unit of water $\alpha\xi$ by the process of which the final result is the substitution in that unit of ξ for χ, that unit must be regarded as passing by a process of continuous change through every value intermediate between $\alpha\chi$ and $\alpha\xi$, an assumption in perfect accordance with what we know of the gradual character of chemical changes, which are not instantaneous events, but events occurring in definite periods of time. This question first comes under our notice when we pass from the consideration of things and of events to the consideration of the chemical relations of things and the chemical relations of events.

Certain relations of the units of matter are the necessary consequence of the nature of those simple chemical events in which they have their origin. If we consider the symbols of the four units of ponderable matter which appear in a simple event of the second order,

$$A(x-a)(y-b)=0,$$

namely,

$$A\,x\,y,$$
$$A\,a\,y,$$
$$A\,x\,b,$$
$$A\,a\,b,$$

it appears that these symbols are the combinations of the letters x, y, a, b, taken two and two, combined with a constant A, and are necessarily derived by substitution the one from the other—$A\,a\,y$ being derived from $A\,x\,y$ by the substitution of a for x,

$A\,x\,b$ from $A\,x\,y$ by the substitution of b for y, and $A\,a\,b$ from $A\,x\,y$ by the substitution of a for x and of b for y. Similarly, in the case of a simple event of the third order,

$$A(x-a)(y-b)(z-c)=0,$$

we have as the symbols of the units which appear in that event

$$\begin{aligned}&A\,x\,y\,z,\\&A\,a\,y\,z,\\&A\,x\,b\,z,\\&A\,x\,y\,c,\\&A\,a\,b\,z,\\&A\,a\,y\,c,\\&A\,x\,b\,c,\\&A\,a\,b\,c,\end{aligned}$$

which contain the combinations of the letters x, y, z, a, b, c taken three and three together, and may severally be regarded as derived from the symbol $A\,x\,y\,z$ by the substitution in that expression, in all possible ways, of a for x, b for y, and c for z. Similar relations obviously prevail between the symbols of the units of matter which appear in simple events of the fourth or any higher order. These relations are at once perceived on the consideration of the general forms of chemical equations. In special instances they are to a certain extent veiled by the identification of the symbols of some among the simple weights, by the exchanges of which the event occurs, and also by the suppression of the chemical symbol 1. As regards the last point it may be observed that we are always at liberty to replace the chemical symbol 1, where it appears as the symbol of a simple weight, by a special symbol (say the symbol ϖ), since this symbol, thus introduced, satisfies all conditions required of the prime factor of a chemical equation. Before interpreting results this symbol is to be put equal to 1. Take, for example, the equation given, Sec. II. (12),

$$\alpha^2\kappa(\chi-1)^3=0;$$

putting $1=\varpi$ we have as the equation to the event in question the homogeneous equation

$$\alpha^2\kappa(\chi-\varpi)^3=0,$$

whence

$$(\alpha^2\kappa)\chi^3+3(\alpha^2\kappa)\chi\varpi^2=3(\alpha^2\kappa)\chi^2\varpi+(\alpha^2\kappa)\varpi^3,$$

the symbols of the units which appear in the event being

$$\begin{aligned}&(\alpha^2\kappa)\chi^3,\\&(\alpha^2\kappa)\chi^2\varpi,\\&(\alpha^2\kappa)\chi\varpi^2,\\&(\alpha^2\kappa)\varpi^3.\end{aligned}$$

So that each symbol may be regarded as derived from the symbol $(\alpha^2\kappa)\chi^3$ by the substitution in all possible ways in that symbol of ϖ for χ. This relation by way of substitution is a universal bond of chemical functions, and prevails among things, among groups of things, and among events. It is desirable to indicate this relation by a name and by a symbol.

(2) Definition:—

Two chemical functions will be said to be congruous to one another in regard to a special substitution if the two functions are of such a nature that they assume the same value when that substitution is effected in them respectively. The value which a function assumes when a special substitution is effected in it will be termed the "residue" of that function in regard to that substitution, and the symbol of the substitution when it appears in this connexion will be termed a "modulus." Further, the symbol $\equiv$ is to be interpreted "is congruous to"; thus, for example, if $f(x)$ be the symbol of a chemical function of the letter x, if $x-a$ be the symbol of the modulus, and R the symbol of the residue,

$$f(x)\equiv \mathrm{R},\quad \text{modulus } (x-a).$$

This expression is termed a chemical congruence, and is to be read $f(x)$ "is congruous to" R for the modulus $(x-a)$.

Similarly we have

$$f(x, y, z, \ldots)\equiv \mathrm{R},\quad \bmod\,(x-a) \bmod\,(y-b) \bmod\,(z-c)\ldots,$$

where R is the value which $f(x, y, z, \ldots)$ assumes when in that function a is substituted for x, b for y, c for z . . .

(3) I proceed to determine the conditions satisfied by functions congruous to a given residue for a given modulus.

Let
$$f(x)\equiv \mathrm{R},\quad \bmod\,(x-a);$$

for x in $f(x)$ we will write $a+x-a$: we have then

$$f(x)=f(a+\overline{x-a}).$$

Now since $f(x)$ is a rational and integral function of x (for there are no other chemical functions), $f(a+\overline{x-a})$ is a rational and integral function of $(x-a)$. Putting then

$$\mathrm{A}_0+\mathrm{A}_1(x-a)+\mathrm{A}_2(x-a)^2+\ldots\ldots\mathrm{A}_n(x-a)^n$$

as the general expression for such a function of $(x-a)$, where $\mathrm{A}_0, \mathrm{A}_1, \mathrm{A}_2, \ldots \mathrm{A}_n$ are functions of a and free from x, and where n is a positive integer, we have

$$f(a+\overline{x-a})=\mathrm{A}_0+\mathrm{A}_1(x-a)+\mathrm{A}_2(x-a)^2+\ldots\mathrm{A}_n(x-a)^n.$$

Now it has been demonstrated (Sec. I. (4)) that every continued product of two or more chemical factors of the form $(x-a)(y-b)\ldots$ is necessarily equal to zero.

Hence

$$A_2(x-a)^2=0,$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

$$A_n(x-a)^n=0,$$

and

$$f(a+\overline{x-a})=A_0+A_1(x-a).$$

Since this last equation is always true, it is true when $x=a$; putting $x=a$,

$$f(a)=A_0;$$

and also since the residue R is the value of $f(x)$, when $x=a$,

$$A_0=R,$$

and

$$f(x)=f(a)+A_1(x-a).$$

To determine A_1 we have

$$A_1=\frac{f(x)-f(a)}{x-a}.$$

This equation again being always true is true when $x=a$; but the limit of the value of

$$\frac{f(x)-f(a)}{x-a},$$

when $x=a$, is $f_1(a)$, where $f_1(a)$ is the first derived function of $f(x)$, that is $\frac{df(x)}{dx}$, in which a is substituted for x. We have therefore for the value of A_1,

$$A_1=f_1(a),$$

and

$$f(x)=f(a)+f_1(a)(x-a).$$

Similarly, in the case of the congruence

$$f(x, y)\equiv f(a, b), \text{ mod } (x-a) \text{ mod } (y-b),$$

we have, regarding y as a constant and developing by the above theorem,

$$f(x, y)=f(a, y)+f_1(a, y)(x-a),$$

and also

$$f(a, y)=f(a, b)+f_1(a, b)(y-b),$$

$$f_1(a, y)=f_1(a, b)+f_{1.1}(a, b)(y-b);$$

whence, substituting these values for $f(a, y)$ and $f_1(a, y)$, we have

$$f(x, y)=f(a, b)+f_{1.0}(a, b)(x-a)+f_{0.1}(a, b)(y-b)+f_{1.1}(a, b)(x-a)(y-b),$$

and

$$f(x, y)=f(a, b)+f_{1.0}(a, b)(x-a)+f_{0.1}(a, b)(y-b).$$

Similarly, considering the congruence

$$f(x, y, z) \equiv f(a, b, c), \quad \bmod (x-a) \bmod (y-b) \bmod (z-c),$$

we have

$$f(x, y, z) = f(a, b, c) + f_{1.0.0}(a, b, c)(x-a) + f_{0.1.0}(a, b, c)(y-b) + f_{0.0.1}(a, b, c)(z-c),$$

and so on, a similar relation holding good in the general case

$$f(x,y,z,v,w,\ldots) \equiv f(a,b,c,d,e,\ldots), \bmod(x-a) \bmod(y-b) \bmod(z-c) \bmod(v-d) \bmod(w-e)\ldots$$

(4) The symbol $\equiv$, which is here adopted as the symbol of "chemical congruence," is used in mathematical investigations of the properties of numbers as the symbol of numerical congruence. Thus the expression $x \equiv a$, mod p indicates to us that the integral number x satisfies the condition given in the equation $x = a + py$, where a, p, y are integers as well as x, so that the difference $x-a$ is divisible without remainder by the modulus p. Now the simplest form of a chemical congruence, $f(x) \equiv f(a)$, mod $(x-a)$, is, in the most exact sense, an "algebraical congruence," for $f(x)$ satisfies the condition $f(x) = f(a) + Q(x-a)$, where $f(x)$, $f(a)$, and Q are respectively rational and integral functions of x and a, so that $f(x) - f(a)$ is divisible without remainder by the modulus $x-a$. In the case of the chemical congruence to two moduli, such as $f(x, y) \equiv f(a, b)$, mod $(x-a)$ mod $(y-b)$, where $f(x, y)$ satisfies the condition $f(x, y) = f(a, b) + A_1(x-a) + B_1(y-b)$, an analogous condition prevails. In this case if we divide the difference $f(x, y) - f(a, b)$ by the modulus $(x-a)$ we have as the remainder of the division $f(a, y) - f(a, b)$, which is divisible without remainder by the modulus $y-b$. It hence appears that a "chemical congruence" for two or more moduli is but a wider kind of numerical congruence, being a comprehensive form in which all such "congruences" are included. The same fundamental idea of congruity is applicable in either case; and as no confusion is likely to arise from the use of the symbol $\equiv$ as the symbol of the chemical relation referred to, which is, indeed, the fundamental relation of the science of chemistry, I have not hesitated to adopt it.

(5) From the various illustrations already given of "simple chemical events," taken together with the explanation given in Section II. of the aggregation of events, the following definition of the "theoretical analysis" of a compound chemical event occurring by any number of substitutions will be readily appreciated.

Definition:—The theoretical analysis of a chemical event occurring by any number of specified substitutions, namely, by the substitution of a for x, b for y, c for z, and so on, will be here said to be effected when all the different chemical events, occurring in any way whatever by these substitutions, are enumerated, the aggregate of which constitutes the event in question; and the real analysis of such an event will be said to be completely effected when all these events are severally realized as independent phenomena, and will be said to be partially effected when two or more of such events or aggregates of such events are so realized.

This analytical problem is presented to us in every chemical congruence.

The congruence

$$f(x, y, z \ldots) \equiv f(a, b, c \ldots), \quad \text{mod}\,(x-a)\,\text{mod}\,(y-b)\,\text{mod}\,(z-c)\ldots$$

asserts that the residue $f(a, b, c \ldots)$ is derived from the function $f(x, y, z \ldots)$ by the substitutions specified in the modules. The problem now to be considered is in what way these substitutions are effected. The solution of this problem is afforded by means of the following theorem.

The demonstration of TAYLOR'S theorem, in which it is assumed that $f(x)$ may be developed in a series of ascending powers of x with integral indices, is entirely independent of the interpretation of the symbols, and is recognized as dependent solely upon those formal properties of symbols expressed in the equations $xy=yx$, $x(y+z)=xy+xz$, known as the commutative and distributive laws. Now in the first part of this Calculus (Part I. Sec. II. (5), (6)) it has been fully demonstrated that the symbols of chemical operations, with the interpretation there assigned to them, satisfy these fundamental conditions. Precisely, therefore, as we are enabled through these properties of those symbols to work with them according to the processes of elementary algebra, so we are equally justified in applying to them the principles of the differential Calculus regarded from this point of view. I shall now prove that if $f(x)$ satisfies the condition given in the congruence

$$f(x) \equiv f(a), \quad \text{mod}\,(x-a),$$

$$f(x)=f(a)+f_1(a)(x-a)+\frac{1}{1.2}f_2(a)(x-a)^2+\frac{1}{1.2.3}f_3(a)(x-a)^3+\ldots+\frac{1}{\lfloor n}f_n(a)(x-a)^n,$$

where $f_1(a)$, $f_2(a)$, $f_3(a) \ldots f_n(a)$ are the first, second, third, and nth derived functions of $f(x)$ in which a is substituted for x.

For let $f(x)$ be any function of x, and in that function let x undergo a variation of such a nature that x becomes $x+\Delta x$; we then have, by TAYLOR'S theorem,

$$f(x+\Delta x)=f(x)+f_1(x)\Delta x+\frac{1}{1.2}f_2(x)\Delta x^2+\frac{1}{1.2.3}f_3(x)\Delta x^3+\ldots+\frac{1}{\lfloor n}f_n(x)\Delta x^n.$$

Now let Δx be that variation which x undergoes when a is substituted for x; we have then $x+\Delta x=a$, $\Delta x=a-x$, and $f(x+\Delta x)=f(a)$. Substituting in the above development $f(a)$ for $f(x+\Delta x)$ and $a-x$ for Δx, we have

$$f(a)=f(x)+f_1(x)(a-x)+\frac{1}{1.2}f_2(x)(a-x)^2+\frac{1}{1.2.3}f_3(x)(a-x)^3+\ldots+\frac{1}{\lfloor n}f_n(x)(a-x)^n,$$

whence, changing a into x and x into a,

$$f(x)=f(a)+f_1(a)(x-a)+\frac{1}{1.2}f_2(a)(x-a)^2+\frac{1}{1.2.3}f_3(a)(x-a)^3+\ldots+\frac{1}{\lfloor n}f_n(a)(x-a)^n.$$

Now among the properties of chemical functions demonstrated in Sec. I. it has been shown that every expression of the form $(x-a)^n$, when $n>1$, is necessarily equal to zero.

We may therefore at once resolve the above equation into the following system of equations:—

$$\text{I.}\quad f(x)-f(a)-f_1(a)(x-a)=0.$$

$$\text{II.}\quad \frac{1}{1.2}f_2(a)(x-a)^2=0.$$

$$\text{III.}\quad \frac{1}{1.2.3}f_3(a)(x-a)^3=0.$$

. . . .

. . . .

$$\frac{1}{\underline{|n}}f_n(a)(x-a)^n=0.$$

The first of these equations is that which has been already given as indicating the condition by which $f(x)$ is related to the residue $f(a)$. The aggregate of the succeeding equations is identical with that equation; and the equations separately considered inform us of the successive steps by which the result indicated in equation I.,

$$f(x)=f(a)+f_1(a)(x-a),$$

is attained.

This method of development is applicable to every chemical function; but if $f(x)$ be a chemical equation of the form $f(x)=0$, it follows from the property of chemical equations demonstrated (Section I. (8)) that $f(a)$ is also a chemical equation; whence

$$f(a)=0,$$
$$f_1(a)(x-a)=0.$$

It is to be observed that in this development of $f(x)$ in ascending powers of the modulus $(x-a)$, the symbols of the units of matter, which appear in the several events of which $f(x)$ is constituted, are resolved into their components according to the classification of those components given in Section III. (3), namely, as "variables," "values," and "constants,"—the "variable" being symbolized as x, the "value" of that "variable" as a, and the "constants" being given in the coefficients

$$f_1(a),\ \frac{1}{1.2}f_2(a),\ \frac{1}{1.2.3}f_3(a),\ \dots\ \frac{1}{\underline{|n}}f_n(a).$$

This classification of the components of the units of matter thus satisfies the conditions required of an accurate method of classification, being both exclusive and exhaustive.

A chemical function of two variables $f(x,y)$ satisfying the condition

$$f(x,y)\equiv f(a,b),\quad \text{mod}\,(x-a)\ \text{mod}\,(y-b)$$

may be developed on precisely the same principles.

Using a similar notation to that previously employed, let

$$f_{1}\ (x,y)=\frac{df(x,y)}{dx},$$

$$f_{0.1}(x,y)=\frac{df(x,y)}{dy},$$

$$f_{1.1}(x,y)=\frac{d^2f(x,y)}{dx.dy},$$

$$f_{2}\ (x,y)=\frac{d^2f(x,y)}{dx^2},$$

$$f_{0.2}(x,y)=\frac{d^2f(x,y)}{dy^2},$$

and generally

$$f_{m.n}(x,y)=\frac{d^{m+n}f(x,y)}{d^mx.d^ny},$$

and let

$$f_{m.n}(a,b)=f_{m.n}(x,y),$$

in which a is substituted for x, and b for y.

Now in $f(x,y)$ let x and y respectively vary, so that x becomes $x+\Delta x$ and y becomes $y+\Delta y$; we then have, by TAYLOR's theorem,

$$\begin{aligned}f(x+\Delta x,\ y+\Delta y)=f(x,y)+f_{1}(x,y)\Delta x+f_{0.1}(x,y)\Delta y+\frac{1}{1\,.\,2}f_{2}(x,y)\Delta x^2\\ +f_{1.1}(x,y)\Delta x\Delta y+\frac{1}{1\,.\,2}f_{0.2}(x,y)\Delta y^2+\frac{1}{\underline{|3}}f_{3}(x,y)\Delta x^3+\frac{1}{\underline{|2}}f_{2.1}(x,y)\Delta x^2\Delta y\\ +\frac{1}{\underline{|2}}f_{1.2}(x,y)\Delta x\Delta y^2+\frac{1}{\underline{|3}}f_{0.3}(x,y)\Delta y^3\ldots+\frac{1}{\underline{|m}\,\underline{|n}}f_{m.n}(x,y)\Delta x^m\Delta y^n,\end{aligned}$$

whence, assuming as before Δx to be the variation which x undergoes when x is substuted for a, and Δy the variation which y undergoes when y is substituted for b, we have

$$\Delta x=a-x,$$
$$\Delta y=b-y.$$

Substituting these values in the above development for Δx and Δy, and again changing in the result a into x and x into a, and b into y and y into b, we have

$$\begin{aligned}f(x,y)=f(a,b)+f_{1}(a,b)(x-a)+f_{0.1}(a,b)(y-b)+\frac{1}{\underline{|2}}f_{2}(a,b)(x-a)^2+f_{1.1}(a,b)(x-a)(y-b)\\ +\frac{1}{\underline{|2}}f_{0.2}(a,b)(y-b)^2+\frac{1}{\underline{|3}}f_{3}(a,b)(x-a)^3+\frac{1}{\underline{|2}}f_{2.1}(a,b)(x-a)^2(y-b)\\ +\frac{1}{\underline{|2}}f_{1.2}(a,b)(x-a)(y-b)^2+\frac{1}{\underline{|3}}f_{0.3}(a,b)(y-b)^3\ldots+\frac{1}{\underline{|m}\,\underline{|n}}f_{m.n}(a,b)(x-a)^m(y-b)^n.\end{aligned}$$

On the principles laid down in the last case, this equation may be resolved into the following system of equations, by which it is adequately represented :—

$$f(x,y)=f(a,b)+f_1(a,b)(x-a)+f_{0.1}(a,b)(y-b),$$

$$\frac{1}{\underline{|2}}f_2(a,b)(x-a)^2=0,$$

$$f_{1.1}(a,b)(x-a)(y-b)=0,$$

$$\frac{1}{\underline{|2}}f_{0.2}(a,b)(y-b)^2=0,$$

$$\frac{1}{\underline{|3}}f_3(a,b)(x-a)^3=0,$$

$$\frac{1}{\underline{|2}}f_{2.1}(a,b)(x-a)^2(y-b)=0,$$

$$\frac{1}{\underline{|2}}f_{1.2}(a,b)(x-a)(y-b)^2=0,$$

$$\frac{1}{\underline{|3}}f_{0.3}(a,b)(y-b)^3=0,$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

$$\frac{1}{\underline{|m}\,\underline{|n}}f_{m.n}(a,b)(x-a)^m(y-b)^n=0.$$

If $f(x,y)$ be a chemical equation, so that

$$f(x,y)=0,$$

$f(a,b)$ is also a chemical equation, so that

$$f(a,b)=0;$$

and, moreover, observing that

$$f_1\ (a,b)(x-a)=f(x,b)-f(a,b),$$

$$f_{0.1}(a,b)(y-b)=f(a,y)-f(a,b),$$

$f(x,b)$ and $f(a,y)$ also being chemical equations, we have

$$f_1\ (a,b)(x-a)=0,$$

$$f_{0.1}(a,b)(y-b)=0.$$

In the two last equations, taken together with the equation $\frac{1}{\underline{|2}}f_2(a,b)(x-a)^2=0$ and the succeeding equations of the system, the theoretical analysis of the event $f(x,y)=0$ is effected, since these equations are collectively identical with the equation $f(x,y)=0$, and in all respects adequately represent that equation.

By the application of these principles we arrive at the following rule for the development of a chemical function $f(x,y,z,v,w,\ldots)$, which satisfies the congruence

$$f(x,y,z,v,w,\ldots)\equiv f(a,b,c,d,e,\ldots),\ \bmod (x-a) \bmod (y-b) \bmod (z-c) \bmod (v-d) \ldots,$$

Develop by TAYLOR'S theorem $f(x+\Delta x,\ y+\Delta y,\ z+\Delta z,\ v+\Delta v,\ldots)$ in ascending powers of Δx, Δy, Δz, Δv, Δw, ... Substitute in the development thus effected for

$\Delta x, \Delta y, \Delta z, \Delta v, \ldots$ respectively $a-x$, $b-y$, $c-z$, $d-v, \ldots$, and in the result change a into x and x into a, b into y and y into b, c into z and z into c, d into v and v into d, and so on. We hence arrive at the development of $f(x, y, z, v, \ldots)$ in ascending powers of the moduli $x-a$, $y-b$, $z-c$, $v-d \ldots$. Proceeding as before we have

$$\begin{aligned} f(x,y,z,v,w,\ldots)=&f(a,b,c,d,e,\ldots)+f_1(a,b,c,d,e,\ldots)(x-a)\\ &+f_{0.1}(a,b,c,d,e,\ldots)(y-b)+f_{0.0.1}(a,b,c,d,e,\ldots)(z-c)\\ &+f_{0.0.0.1}(a,b,c,d,e,\ldots)(v-d)+\&c., \end{aligned}$$

together with a system of equations of the form

$$\frac{1}{\underline{|m}\,\underline{|n}\,\underline{|p}\,\underline{|q}\ldots}f_{m.n.p.q}(a,b,c,d,\ldots)(x-a)^m(y-b)^n(z-c)^p(v-d)^q\ldots=0.$$

Reasoning as before, if $f(x, y, z, v, \ldots)$ be a chemical equation, we have

$$\begin{aligned} &f(x,y,z,v,\ldots)=0,\\ &f(a,b,c,d,\ldots)=0.\\ &f_1(a,b,c,d,\ldots)(x-a)=0,\\ &f_{0.1}(a,b,c,d,\ldots)(y-b)=0,\\ &f_{0.0.1}\ (a,b,c,d,\ldots)(z-c)=0,\\ &f_{0.0.0.1}(a,b,c,d,\ldots)(v-d)=0. \end{aligned}$$

In these last equations, taken together with the equations previously referred to of the second and higher orders, the theoretical analysis of the event $f(x, y, z, v, \ldots)=0$ is effected, these equations collectively representing, for all purposes, the equation $f(x, y, z, v, \ldots)=0$.

As a necessary preliminary to the analysis of a compound chemical event, the substitutions must be specified in reference to which the analysis is to be effected. We can then proceed by means of the preceding theorem to resolve the event $f(x, y, z, \ldots)=0$ into its constituents, which consist essentially of two groups, the event $f(a, b, c, \ldots)=0$, which does not occur by the substitutions in question, and the system of simple events enumerated in the various terms of the development, occurring in all possible ways by these substitutions, an analysis which, as we have said, is absolutely exhaustive.

(6) From these considerations we arrive at a more exact definition than has hitherto been open to us of a normal chemical equation, and of the position which it occupies in the general algebraical system. We have seen that in the case of any chemical equation $f(x, y, z, \ldots)=0$,

$$\begin{aligned} &f(a,b,c,d,\ldots)=0,\\ &f_1(a,b,c,d,\ldots)(x-a)=0,\\ &f_{0.1}\ (a,b,c,d,\ldots)(y-b)=0,\\ &f_{0.0.1}(a,b,c,d,\ldots)(z-c)=0,\\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

Now, since these equations are always true, they are true when $a=1$, $b=1$, $c=1$; whence

$$
\begin{aligned}
&f(1, 1, 1, 1\ldots)=0,\\
&f_1(1, 1, 1, 1\ldots)(x-1)=0.\\
&f_{0.1}(1, 1, 1, 1\ldots)(y-1)=0,\\
&f_{0.0.1}(1, 1, 1, 1\ldots)(z-1)=0.\\
&\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot
\end{aligned}
$$

Hence it follows that if in any chemical equation $u=0$ the prime factors by which the equation is expressed be severally and simultaneously put equal to 1 (which is the only numerical value of chemical symbols), that equation vanishes, and also if we differentiate that equation once in regard to any one (and every one) of these factors, and in the result of that differentiation put all the prime factors severally and simultaneously equal to 1, that differential coefficient also vanishes*. A chemical equation, therefore, may be defined as an equation which possesses these properties.

(7) Further, these considerations supply us with a general and comprehensive theory of the mode of occurrence of chemical events. It has been shown in the first part of this Calculus that in every chemical equation the symbols of the units of matter are expressed by the smallest possible number of prime factors when expressed by the factors $\alpha, \xi, \theta, \chi, \beta, \omega, \nu, \phi, \ldots$ So that every chemical equation regarded as a member of the general system is of the form

$$f(\alpha, \xi, \theta, \chi, \beta, \omega, \nu, \phi, \ldots)=0.$$

Now every such equation necessarily vanishes when the prime factors $\alpha, \xi, \theta, \chi, \ldots$ are severally and simultaneously put equal to 1, and also admits of being developed in ascending powers of the moduli $(\alpha-1), (\xi-1), (\theta-1), (\chi-1), \ldots$ Hence every chemical event, without exception, may be considered to occur by the transferences of the simple weights $w(\alpha)$, $w(\xi)$, $w(\theta)$, $w(\chi)$, . . ., and admits of being resolved into a set of "constituent simple events" severally occurring in various ways by these same transferences; and if we are to refer all chemical events to one set of causes, so as to bring them under one law, these transferences are the only set of causes to which these phenomena can be referred. Thus, as we are led to contemplate the resolution of the units of matter into the "system of simple weights" $w(\alpha)$, $w(\xi)$, $w(\theta)$, $w(\chi)$, . . . as the ultimate limit of the analysis of those units, so here another and not altogether dissimilar problem is suggested to us—namely, the actual analysis of all chemical events into a system of simple constituent events occurring in various ways by the transferences to and fro among the units of matter and space of these same "simple weights." We are far indeed from this ultimate ideal goal, but may yet recognize it on the far horizon as the limit of our speculations.

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* This latter property of a chemical equation has already been demonstrated in Part I. Section V., and forms indeed the basis of this Calculus. It here reappears in another form. The condition $f(1, 1\ldots)=0$ is (as is obvious) a necessary property of every chemical equation when brought to the normal form in the way indicated in Section I. (3). But it may be noticed that the analysis of a chemical event is quite independent of the prior reduction to this form, which is effected in the course of the development itself. Take, for example, the equation given in I. Section IV. (1), $xy=x+y$, the fundamental chemical equation. The equation in this form does not inform us of the process by which the transformation in question is effected. But putting $f(x,y)=xy-x-y$ and developing we have $f(1, 1)=-1$, $f_{1.0}(1, 1)=0$, $f_{0.1}(1, 1)=0$, $f_{1.1}(1, 1)=1$, and $f(x, y)=-1+(x-1)(y-1)$ or $f(x, y)+1=(x-1)(y-1)$, the equation $f(x, y)+1=0$ being the equation $xy=x+y$ reduced to the normal form, namely $1+xy=x+y$.

(8) We are thus led to regard many events of an apparently simple character as constituted of numerous other events, some realizable, others not, the concurrence of which results in those events. To some, even when demonstrated, this may appear a complex view. But the complexity is not real. It is in truth the simplest possible doctrine. We may compare the aggregates of simple events of which compound events are constituted to the aggregate of the repeated blows of the hammer, by which (each falling with a certain force and in a certain requisite direction) two or more pieces of iron are welded together and shaped to a determinate form. What is here effected is to specify the kind of blows which are required and the number of blows of each kind. Now it may be asked, since every chemical event is here referred to one set of causes, and regarded as occurring by the transferences of the "simple weights" $\alpha, \xi, \chi, \ldots$, How are we to interpret such expressions as $(x-a)$, $(xy-ab)$, when they occur among the factors of chemical equations? Such expressions apparently involve a change in our point of view; for $(x-a)$ is not the symbol of the "transference" of x, but of the substitution of one "simple weight," a, for another, x, and $(xy-ab)$ is not the symbol of any substitution of one simple weight for another, but is the symbol of the substitution of a compound weight ab for a compound weight xy. To this it may be replied that we are here considering results; that the expression $(x-a)$ means the occurrence of a phenomenon of which the result is the substitution of a for x. But it does not inform us of any particular way in which this result is attained. And, in fact, this result may with equal justice be considered as the aggregate of the two transferences $(x-1)-(a-1)$ as a single substitution. Such an expression, therefore, as $(x-a)(y-b)$ is always to be regarded as an abbreviated form of expression for the aggregate

$$(x-1)(y-1)-(x-1)(b-1)-(a-1)(y-1)+(a-1)(b-1).$$

Similarly, developing the expression $(xy-ab)$, we have

$$xy-ab=(x-1)+(y-1)+(x-1)(y-1)-(a-1)-(b-1)-(a-1)(b-1),$$

so that the event symbolized (for example) as $(xy-ab)(z-1)$ is to be regarded as an abbreviated expression for the aggregate of events,

$$(x-1)(z-1)+(y-1)(z-1)+(x-1)(y-1)(z-1)-(a-1)(z-1)-(b-1)(z-1)$$
$$-(a-1)(b-1)(z-1).$$

It is my intention to lay before the Society, in a further communication, a set of examples illustrative of the application of the principles of this Calculus to the discussion of special chemical events.

III. *On the Structure of a Species of* Millepora *occurring at Tahiti, Society Islands.*

By H. N. Moseley, *F.R.S., Fellow of Exeter College, Oxford, Naturalist to the 'Challenger' Expedition. Communicated by Professor* Sir C. Wyville Thomson, *F.R.S., Director of the Civilian Scientific Staff.*

Received March 6,—Read April 6, 1876.

[Plates 2 & 3.]

In a paper treating mainly on the structure of the *Heliopora cærulea*, which was communicated to the Royal Society in the autumn of last year (1875), I gave a short account of the results at which I had arrived from the examination of two species of *Millepora* obtained at Bermuda and at the Philippines, and expressed my intention of further prosecuting the subject at the Sandwich Islands and Tahiti, should material be forthcoming.

At Honolulu no *Millepora* was met with; and this form apparently does not occur at the Sandwich Islands, the water being too cold for it. At Tahiti a *Millepora* is very abundant on the reefs in from one to two feet of water, and is very conspicuous because of its bright yellow colour.

I failed in an attempt to procure the animals of this species in an expanded condition; but my colleague, Mr. J. Murray, succeeded on two occasions, and on the second occasion showed me the expanded zooids, and handed the living specimens over to me for examination. I am greatly indebted to Mr. Murray for having thus afforded me the opportunity of studying the zooids of *Millepora* in the expanded condition, and I do not think that I should ever have succeeded in arriving at a satisfactory knowledge of their structure without this aid. Mr. Murray further, who had had better opportunities of observing the living coral than I, first drew my attention to the fact that the central zooid of each system had a mouth. No species of *Millepora* appears hitherto to have been known to occur on the reefs of the Society Islands. In Dana's work on Corals[*] no *Millepora* is mentioned as occurring at Tahiti, and this locality is not given for any species of *Millepora* by MM. Edwards and Haime. It is impossible to determine with any certainty the species of such a form as *Millepora* without access to museum collections. The Tahitian species, of which the structure is here described, resembles closely in form *M. tuberculosa* (*M. gonagra*) figured by MM. Edwards and Haime[†]. Like this species it never forms foliaceous expansions, but is tuberculate and irregular in shape, and often encrusting, commonly overgrowing the dead fronds of *Lophoseris cactus*, which is a principal component of the Tahitian reefs. The present species seems, however, to differ from *M. tuberculosa* in that its calicles are disposed over the surface of the corallum in well-marked and separated systems, and in this respect is more closely allied to *M. plicata*, *M. foliata*, and *M. Ehrenbergii* as described by MM. EDWARDS and HAIME*.

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[*] United States Expl. Exped. vol. vii. Zoophytes, by J. D. Dana. Philad. 1846.

[†] Hist. Nat. des Coralliaires, pl. F 3. figs. 1^a, 1^b.

In the fresh condition the growing tips of the lobes of the coral are of a bright gamboge-yellow colour, which shades off into a yellowish brown on the sides and bases of the lobes. The expanded zooids have the appearance of a close-set pearly white down upon the surface of the corallum.

The zooids seem to be somewhat capricious in the matter of expansion. I made one attempt to obtain them expanded, in which I carefully cut off masses of the coral by means of a chisel, and transferred these to a glass vessel without lifting them above the surface of the water. The zooids did not expand. Mr. MURRAY succeeded on both occasions on which he collected specimens, although he exposed his to the air for a moment on transferring them to a vessel. DANA, POURTALES, and others speak of the peculiar difficulties attending the observation of the animals of *Millepora*. The corallum is so hard that it is almost impossible to break off a small flake without damaging the soft structures to such an extent that the animals fail to expand. The animals, as expanded on larger masses of the corallum, can only be examined with very low powers of the microscope, which, on account of the very small size of the animals, are unable to show sufficient details in their structure. A momentary view of one mouthless zooid was, however, obtained under HARTNACK's objective No. 4, eyepiece No. 3. I obtained the view of the expanded zooids only on the morning on which H.M.S. 'Challenger' was steaming out of Papute Harbour. The animals remained expanded about two hours, but the motion of the ship interfered considerably with the investigation of them.

A summary of the literature extant on the subject was given in the paper on *Heliopora cærulea* above referred to. It is needless here to recapitulate.

Methods.

Sections of the corallum were prepared in the usual manner by grinding. Portions of the living coral were placed in various solutions for subsequent examination, viz. in absolute alcohol, chromic acid, and glycerine. Portions were further treated with osmic acid, and then transferred to glycerine or absolute alcohol. Fragments of the hardened coral were subsequently decalcified with hydrochloric acid, and the residual soft structures were either mounted entire for examination, or cut in the usual manner into fine vertical and horizontal sections. The sections were stained with carmine or magenta. The specimens hardened in osmic acid, and decalcified after subsequent immersion in absolute alcohol, yielded the best histological results. Those which had been hardened in absolute alcohol alone gave the best results as to the coarser anatomy. The specimens preserved directly in glycerine preserved most perfectly the forms of the several histological elements, and especially yielded good preparations of the thread-cells, preparations of which are best procured by grinding up between two glass slides a zooid and its immediately surrounding calcareous bed, removed with the point of a scalpel. A view of the structure unacted upon by acids is thus obtained. The specimens placed in chromic acid were of little service for sections, owing to a thick crystalline deposit of sulphate of lime which formed upon them in the solution; but they showed best, on the under surface of the decalcified superficial film, the ramifications of the soft parts of the hydrophyton.

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* NOTE, Feb. 28, 1877.—I have submitted a specimen of the corallum of the Tahitian species here treated of to my friend Dr. F. BRÜGGEMANN, who is at present engaged in arranging and determining the collection of corals in the British Museum, and he has kindly determined it for me to be *Millepora nodosa*, Esper, Pflanzenthiere, vol. i. p. 199, Millep. pl. I. (1791); mentioned by M. EDWARDS under *M. gonagra*, Hist. Nat. des Cor. iii. p. 230.

Structure of the Corallum.

The structure of the corallum is illustrated on Plate 2. The corallum has a widely spread encrusting base covering rocks, dead corals, &c., and at its surface presents a series of projecting, short, irregular tubercles and lobules, which never rise to any considerable height. Fig. 3 represents the appearance of two lobules of the corallum and a portion of a third, enlarged two diameters. The surface of the lobules is uneven and covered with slight rounded elevations. The calicles, or pores, of the zooids are dispersed over the entire surface both of the lobules and of the flatter encrusting portions of the corallum, being absent only at the tips of some of the lobules, which are possibly those that are in rapid growth. The calicles are disposed in irregularly circular groups, a larger calicle being in the centre of each group or system, with usually from five to eight smaller calicles arranged around it. These systems of calicles often occupy small rounded prominences on the surface of the corallum, and in parts of some specimens almost every system appears to have its separate small prominence. In some regions of the corallum the systems are scarcely defined, the calicles appearing irregularly placed; but such an arrangement is only exceptional in the present species. An entire system of calicles has been accurately drawn for me by Mr. J. J. Wild, and is represented in Plate 2. fig. 4, enlarged eighty diameters. The outlines of the calicles are seen to be extremely irregular; their cavities are encroached upon in all directions by projections of the contorted trabecular cœnenchymal tissue of the corallum. The larger central calicles of the systems measure about 1·5 millim. in diameter.

The main mass of the corallum is composed of trabeculæ of dense calcareous matter which forms a spongy-looking mass traversed in all directions by tortuous canals. In some species of *Millepora* the corallum is much more dense than in the Tahitian one, and in these might rather be described as a compact mass in which a series of tortuous channels are excavated for the reception of the soft structures. In such species of

Millepora, in finely ground sections of the corallum, the tortuous canals become filled with opaque débris, and show out, when the section is viewed by transmitted light, dark on a light ground. In a species of *Millepora* obtained at Zamboangan the corallum was of this nature. The appearance presented by a thin section of its corallum is shown in Plate 2. fig. 7. In *Millepora alcicornis* and in the Tahitian species the canal-systems and trabeculæ of calcareous matter seem to form equally complex interpenetrating meshworks. The canal-systems present in the coralla of the species of *Milleporidæ* are perfectly characteristic structures, and distinguish the coralla formed by these Hydroids most clearly from all other recent coralla whatever*. They of course correspond to, and in the recent state contain, the ramifications of the soft parts of the hydrophyton. The canals form regular branching systems with main trunks which give off numerous branches, from which arise secondary branches, and from these again smaller ramifications. The whole canal-system is connected together by a freely anastomosing meshwork of smaller vessels, and communicates freely by numerous offsets with the cavities of the calicles. In Plate 3. fig. 12 part of one of these canal-systems is shown, being there drawn from a decalcified specimen, and thus representing the soft tissues which in the recent state occupied corresponding calcareous canals. In Plate 2. fig. 6 a secondary branch of one of the canals is seen to communicate with a calicular cavity, C. The course of the smaller vessels being tortuous, only short lengths of them are exposed in the remainder of this section. Similar secondary branches are seen in vertical section in Plate 2. fig. 5, B, B.

Where a *Millepora* encrusts foreign bodies, the investing film of corallum formed is usually extremely thin. At Bermuda, *Millepora alcicornis* is frequently found encrusting glass bottles thrown into the harbours. The film of corallum can, in such specimens, easily be detached in flakes from the glass, and does not measure more than from $\frac{1}{8}$ to $\frac{1}{5}$ of a millimetre in thickness. In the same manner at Bermuda the dead fans of a *Gorgonia* are found entirely encrusted with a thin film of *Millepora*, so thin that the fenestrations of the horny meshwork of the *Gorgonia* are not obliterated. Such thin encrusting films, if obtained in the living condition, would, no doubt, be excellently adapted for the study of the soft parts of *Millepora*, since they are thin enough to transmit a considerable amount of light. When dead and dry they show extremely well the ramifications of the canal-systems and their connexions with the calicles. In such films the calicles, larger and smaller, are fully developed, though necessarily very shallow; and it is evident that such a thin film of corallum is all that is absolutely necessary for the existence of the *Millepora*, and, in fact, in all *Milleporæ* it is such a thin film only which is actually living, covering the surface of the corallum. In a *Millepora* forming tubercular or ramified masses a superposition of a series of such films takes place and constitutes the coral mass.

In the films encrusting bottles the under surface in contact with the glass is perfectly continuous and highly polished, and is exactly moulded on the surface of the glass, reproducing casts of the most minute splinterings or scratchings.

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* Note, June 1877.—Excepting the coralla of the Stylasteridæ, since found by me to be also Hydroids and not Anthozoans (Proc. R. Soc. no. 172, 1876, p. 93).

In homology with this continuous layer, layers more or less continuous occur in the more massive coralla, appearing in vertical sections as lines of calcareous matter running parallel to the surface of the corallum, and indicating successive stages of growth; and the tubercles of which the mass of the Tahitian *Millepora* is made up, when cut through vertically to the surface, show a series of such lines of growth following the contour of the surface. It is in connexion with these layers that are developed the successive transverse laminæ or tabulæ which divide the cavities of the calicles into a series of chambers (Plate 2. fig. 5). As the corallum is extended in growth at certain intervals, possibly after each period of generative activity, a tabula is formed, reducing the depth of the calicle and shutting off the living tissue from the abandoned dead structures below. The larger canals and their branches ramify in planes parallel to the surfaces of the corallum, being confined within each successively added thin layer of the corallum, and never take a vertical course leading from the depths of the coral to the surface. A free vertical communication is, however, established by the smaller vessels (Plate 2. fig. 5). In the thin films of *Millepora alcicornis* the trabeculæ of hard tissue run with remarkable uniformity in straight lines parallel to one another, whilst the main canals cross them with a serpentine course.

Histological Structure of the Corallum.

In histological structure the hard tissue composing the corallum of *Millepora* seems to resemble closely that of the coralla of *Heliopora* and most Anthozoa. It is composed of lamellæ of fibro-crystalline calcareous matter (Plate 2. fig. 8), the fibres of the superposed lamellæ crossing one another at all angles in the mass. In some places, in thin sections of the corallum, the appearance shown at *a* (Plate 2. fig. 8) is clearly to be seen. The calcareous fibres of the hard tissue terminate towards a cavity in the corallum as a series of short points, seeming to show a composition of the hard tissue out of definite rod-like elements. Such an appearance is only to be met with sparingly, and possibly occurs at spots where the corallum was in active growth. The hard tissue is bored in all directions by parasitic vegetable organisms (Plate 2. figs. 6 & 8).

Chemical Composition of the Corallum.

Although the animals forming the corallum of *Millepora* differ so widely from those by which all other corals are secreted, their coralla appear to agree in chemical composition with those of other corals as closely as they do in histological structure. Analyses of the coralla of two species of *Millepora* are given by Professor DANA. One is an analysis of *Millepora tortuosa* from the Fijis, by Mr. LILLIMAN, Jun.* The composition was found to be as follows:—

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* 'Structure and Classification of Zoophytes,' by J. D. DANA, A.M. Philadelphia: LEA and BLANCHARD, 1846. Appendix, p. 130.

Carbonate of lime	94·226
Phosphates and fluorides . . .	1·200
Organic matter	4·574

Mr. S. P. SHARPLES* found the corallum of *Millepora alcicornis* to consist of:—

Carbonate of lime	97·46
Phosphate of lime	0·27
Water and organic matter . . .	2·4

There is no marked difference between these results and those obtained from ordinary corals.

Structure of the Soft Parts.

Structure of the Zooids.—The calicles of the *Millepora* are occupied by two kinds of zooids. In each system of calicles the central larger one is occupied by a short and broad zooid provided with a mouth, whilst the surrounding smaller calicles lodge longer and more slender zooids which have no trace of mouth. A system of expanded zooids is shown in Plate 3. fig. 9, one of the mouthless zooids being omitted in the drawing in order to show the central mouthed one more clearly.

The mouth-bearing zooids are much shorter and broader than the mouthless ones. They were not directly measured, but were estimated to be about ·5 millim. in height. They are cylindrical in form, with a short conical hypostome, and four, five, or six tentacles arranged equidistantly in one whorl just below the short hypostome. The tentacles consist of a short, stout, cylindrical stem, with a spheroidal knob-like tip composed almost entirely of thread-cells. At the summit of the hypostome is the mouth, which in the living expanded animal has a conspicuous glistening white appearance—no doubt because light is strongly reflected by the large gastric cells which surround the aperture.

The mouth-area is circular in outline (in *M. alcicornis* quadrangular sometimes), Plate 3. fig. 11, MZ. The circular area is occupied by a series of large, elongate, transparent gastric cells, which are disposed in a radiating manner around the centre of the area. The actual mouth-orifice takes the form either of a threefold or cruciform slit between the gastric cells. The gastric cells (Plate 3. fig. 15) are elongate, irregularly cylindrical in form, and transparent and bladder-like in appearance, and without any trace of a nucleus. They line the internal cavity of the zooid for at least one third of its length, but to what extent exactly was not ascertained. They appear to be closely similar to the pyriform cells described by ALLMAN, and figured by him as occurring in *Gemmaria implexa*†. They are here termed gastric, because the fact that they occur only in the mouthed zooids seems to render it probable that they exercise a digestive function. The mouthless zooids are long and slender in comparison with the mouthed ones. They differ very much in length, as will be seen from the figure; the longest of them measure about 1½ millim. in length. They are cylindrical in form, tapering towards the upper extremity. They have no trace of a mouth, nor of any of the gastric cells of the mouthed zooids in their body-cavity. They bear tentacles at irregular intervals from near the bases to the summits of their bodies. The tentacles are very variable in number; some zooids have only five tentacles, whilst all numbers from 5 to 20 (and possibly, in exceptional cases, a slightly greater number) occur in others. From 12 to 15 is the most usual number. The tentacles consist of a cylindrical stem, longer and more slender than in the mouthed polyps, and a spheroidal tip resembling that of the tentacle of the mouthed zooid, but smaller. The body of the zooid terminates sometimes in two, sometimes in three tentacles, springing from a common point.

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* 'Corals and Coral Islands,' by J. D. DANA (London, 1872), p. 105.

† ALLMAN, 'Gymnoblastic and Tubularian Hydroids,' pl. viii. fig. 5.

The mouthless zooids expand far more readily and quickly than the mouthed zooids, of which latter it is comparatively difficult to obtain a view in the expanded condition. The short mouthed zooids appear to remain perfectly quiescent when expanded, whilst the mouthless zooids are in constant serpentine motion. The mouthless zooids carry their bodies seldom extended straight, but usually bent in several curves; they appear to bend over towards their mouthed zooid from time to time, as if to convey food. All the zooids are retracted on alarm with remarkable suddenness, disappearing entirely within the calicles.

When a portion of the coral has been placed living in reagents, it is found, when hardened, to be bristling all over with sheaves of threads shot from the thread-cells around the mouths of the calicles. By some accident, on one small portion of a coral placed in absolute alcohol, the mouthless zooids all remained partially protruded. This was only over a small area of about ½ of a square inch in dimensions, enough to yield a single microscopical preparation. From a very large quantity of the coral prepared in an exactly similar manner, no second preparation could be obtained, though it was all searched over carefully for similarly expanded zooids. This fact, however, shows that perhaps it might have been possible to obtain a larger quantity of expanded zooids in the hardened condition by the gradual addition of alcohol or fresh water to the sea-water in which the living animals were expanded, or by some similar means; or perhaps by sudden addition of osmic-acid solution as recommended by F. E. SCHULZE[*].

The body of the zooids, when seen in transverse section, is found to consist (fig. 15) of an ectodermal layer, beneath which is a layer of membrane, and an internal mass of endodermal cells. The ectodermal layer, as studied in sections of hardened specimens, appears to consist of well-defined cells, most of which contain small thread-cells, whilst some contain simple nuclei. The membranous layer is apparently structureless; it extends throughout the body and tentacles. Beneath the membranous layer, and in close union with it, are the muscular structures to be presently described, and within these, in the case of the mouthed zooids, are, in the upper region of the body, the gastric cells already described. The structure of the endoderm in the lower part of the body of the mouthed zooids, and in the mouthless zooids, was not well ascertained.

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[*] Anleitung zu wissenschaftlichen Beobachtungen auf Reisen. Herausgegeben von C. NEUMAYER, Hydrograph der kaiserlichen Admiralität. Berlin, 1875. Wirbellose Seethiere von K. MÖBIUS, p. 424.

In transverse hardened sections the body-cavity is seen to be entirely filled with the pigmented yellow cells, which also fill the canals of the hydrophyton. In the tentacles of the mouthless zooids, however, of which a glance was obtained under a high power, the transverse lines or apparent septa, so characteristic of the Hydroids (fig. 13), and considered by ALLMAN to be in reality the opposed walls of large adjacent endodermal cells, were clearly seen, and also multiramified amœboid-looking corpuscles occurring in the endoderm (RC, fig. 13), and resembling those figured by ALLMAN as occurring in the tentacles of *Coryne pusilla*, and considered by him to be the nuclei of the large endodermal cells*.

The body-cavities of the zooids were seen in the living condition to be filled with the yellow pigmented cells, and a few of these cells were seen occasionally to penetrate a short distance into the cavities of the tentacles, which cavities are continuous by widely open mouths with those of the bodies of the zooids. Ciliation of the somatic cavities could not be seen. The spheroidal heads of the tentacles are composed of masses of closely-set thread-cells of various sizes and stages of development, but all of one peculiar kind (Plate 2. fig. 2), the larger ovoid thread-cells never occurring in them. A thin hyaline, apparently structureless ectodermal layer extends between these agglomerated thread-cells, its marginal outline not being circular but depressed in short curves between the somewhat projecting tips of the cells (Plate 3. fig. 13).

The mouthed zooids, when retracted, viewed directly from above, show the mouth in the centre, and four, five, or six tentacles arranged at equal distances around. The mouthless zooids, when retracted, have their tentacles closely drawn together, so as to form a hemispherical mass composed of the closely-set spheroidal tips of the tentacles (Plate 3. figs. 10 & 11). It can easily be understood how a vertical section through such a mass of retracted minute tentacles would give the appearance of a large compound tentacle, the small tentacles appearing to constitute the pinnæ. I was misled by such a preparation; and in my paper upon *Heliopora cærulea*, lately presented to the Royal Society†, I stated my belief that the tentacles of *Millepora* would prove to be compound.

The body-cavities of the zooids terminate inferiorly in blind ends at the bottoms of the calicles, but are continued outwards at their bases in all directions into the canals of the hydrophyton, which join them all around, being disposed in an irregularly radiate manner (Plate 3. figs. 10 & 14).

Muscular fibres, having a longitudinal disposition, are extremely well developed in the zooids. They arise for the most part in bundles from the radiating vessels of the hydrophyton, which spring from the bases of the zooids, and pass up the walls of the bodies of the zooids, extending in the mouthed zooid nearly as far as the mouth. In the contracted zooids, when viewed directly from above or below, they have necessarily a radiate disposition, as shown in fig. 14. Not all the fibres are gathered into the bundles; but some sparsely spread ones occupy the interval between these bundles, maintaining a like radiate course. The bundles may be traced for a considerable distance along the

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[*] ALLMAN, 'Gymnoblastic and Tubularian Hydroids,' pl. iv. fig. 3.

[†] Phil. Trans. vol. 166. pt. 1.

radiating vessels. In vertical sections from osmic-acid preparations the muscular elements can be observed as isolated excessively fine fibres (as far as was seen, without nucleus), which appear to be distinct from the membranous layer of the zooid, though in close relation with it. These bundles of longitudinal fibres are plainly to be seen in preparations of all kinds, and no doubt it is to their presence that the zooids owe their power of almost instantaneous retraction. In several osmic-acid preparations an appearance indicating the existence of a set of circularly-directed fibres lying externally to the longitudinal fibres, or possibly of circular fibrillation of the membranous layer, was seen, but the existence of such structure was not determined with certainty.

Structure of the soft parts of the Hydrophyton.

The canals and spaces within the calcareous corallum are occupied, as has already been stated, by a network of soft tissue. This, together with the hard parts, constitute what, according to ALLMAN's nomenclature, must be termed the hydrophyton, or "common basis by which the several zooids of the colony are kept in union with one another." In the case of *Millepora* the hydrophyton appears from its structure to be homologous with the hydrorhiza of other hydroids, the hydrorhiza here being extraordinarily developed, so as to form a thick network surrounding cavities into which the zooids are retracted, and forming by means of its ectoderm a massive calcareous structure. Only a thin layer at the surface of the coral is living. This layer separates from the underlying dead matter when the coral is decalcified in acids, and appears as a soft membrane about ·5 millim. in thickness. When the entire corallum is dissolved away there remains besides this membrane only a greenish gelatinous mass, which consists of the mycelium and spores of the parasitic organisms, which were the sole living occupiers of its deep parts. The living part of the hydroid seems to be entirely confined, as is the case in *Heliopora cærulea*, to the region superficial to the last-formed tabulæ.

The soft parts of the hydrophyton consist of a series of ramifying canal-systems, which occupy in the recent condition the canals already described as existing in the corallum. The branches and secondary branches of the canals are joined by a complex network of smaller vessels, which join in all directions the body-cavities of the zooids (Plate 3. fig. 12), and thus maintain a vascular connexion between the various zooids of the colony of the freest character. In some cases comparatively large tertiary branches of the canals join the zooid-cavities directly. The large main canals run sometimes for long distances, and in a species of *Millepora* obtained at Zamboangan, Philippines, are plainly visible to the naked eye on the surface of the corallum, extending sometimes for as great a distance as 1½ inch. The ramifications of the hydrophyton are best seen on the under surface of the superficial living film decalcified in chromic acid and viewed by reflected light. The appearance presented in such a preparation is accurately represented in Plate 3. fig. 12. The appearance of the soft parts of the hydrophyton, as seen in vertical section, is shown in Plate 3. fig. 10. In the more superficial region of the living layer the elements of the network take a direction more or less vertical to the surface. The horizontally directed main canals and their branches lie near the under surface of the layer on a level with bases of the zooids. The histological structure of the hydrophyton is shown in Plate 3. fig. 16. The canals and vessels forming the network are composed of an ectodermal layer, with a membranous layer developed beneath it, and of endodermal lining.

The ectodermal layer consists, in the greater part of the network, of fusiform cells with a finely granular appearance, and a well-defined oval nucleus, but with the cell-boundary often very indistinct. These cells form in some places much thicker layers than in others. Towards the most internal part of the hydrophyton they become entirely lost, their place being taken on the surface of the thin-walled cyst-like innermost elements of the network by a thin layer of structureless protoplasm (Plate 3. fig. 16, B).

Narrow strings of this ectodermic protoplasm (Plate 3. fig. 16, S) cross over here and there between adjacent vessels of the innermost part of the network, being possibly the remnants of effete vessels. The ectoderm covering the parts of the network near the surface is much thickened and modified, a large proportion of its cellular elements being there found converted into the parent-cells of thread-cells, and being thus inflated and occupied by thread-cells in all stages of development. At the actual surface the ectodermal cells undergo still greater modification, forming a superficial layer of elongate prismatic transparent cells, which shows on the surface a series of irregularly hexagonal areas corresponding with the summits of the cells. These cells contain oval nuclei and thread-cells of both kinds in various stages of development. The most superficial film showing the hexagonal areas separates often in osmic-acid preparations as an exceedingly thin membrane, as is shown on the right-hand side of Plate 3. fig. 16. There is some uncertainty as to the exact structure of the superficial layer of the ectoderm. The figures represent what, after a careful investigation, was concluded to be the arrangement existing. The layer is seen well only in preparations from specimens hardened in osmic acid. The lateral boundaries of the prismatic cells were never seen well defined, but the polygonal areas corresponding with their summits were seen well in various preparations. It could not be demonstrated with certainty that this layer extends uniformly all over the external surface of the hydrophyton. It is extremely transparent, and difficult to trace in preparations viewed from the surface, over the cavities caused by the removal of the hard parts by decalcification. The exact arrangement of the superficial layer in its connexion with the mouths of the calicles and zooids is also somewhat uncertain. The layer certainly is prolonged into the calicular cavities, and contains the mass of large oval thread-cells which surrounds each zooid. In most preparations the zooids are far retracted through the action of reagents, and the mouths of the calicles are closed above by a layer of tissue, which shows a radiate striation or slight plaiting around a very small circular central orifice, which orifice leads down a short tubular cavity formed by the superficial layer drawn everywhere inwards to the retracted zooids of the calicle. The layer of tissue thus contracted over the calicle nearly or sometimes completely closes it, and thus usually the zooid can be seen in preparations in which the hydrophyton is viewed from the surface only by focusing the objective into the depths of the tissue. The orifices of the calicles of retracted zooids were unfortunately not carefully examined in the living condition of the coral; hence it is uncertain whether the superficial tissue contracts in this manner in the living condition so as to close the orifice of the calicle and protect the zooid, or whether such extreme contraction occurs only through action of reagents. It seems probable that it does occur in the living animal, since by its means the masses of large thread-cells are brought as a protection directly between the zooid and the exterior. Just as in one small portion of the coral the zooids died in the expanded condition, so more often, in certain specimens, they die and are preserved with the superficial ectodermal layer not closed in over the mouth of the calicle, but with the calicle open and their retracted tentacles remaining fully exposed to view from above. In Plate 3. figs. 10 & 11 both zooids figured are shown in this latter condition. The connexion between the superficial ectodermal layer within the calicle and the adjacent vascular network of the hydrophyton was not made out. It represents probably merely the largely developed ectodermal layer of that part of the network, but the connexion not having been seen is not indicated in Plate 3. fig. 10. The superficial layer of the hydrophyton being a special development of the ectodermal cells of the vascular network, and the interspaces in this network being occupied by calcareous trabeculæ, it follows either that the tips of the trabeculæ at the surface of the hydrophyton must be directly exposed, or that the superficial ectodermal cells of the network must close in over them. The latter arrangement seems to occur; and in vertical sections of the decalcified hydrophyton numerous spaces left by removed calcareous structures are seen in the superficial ectodermal layer (see Plate 3. figs. 10 & 16), with the ectodermal cells arching over to cover them. I should have had no doubt in this matter had I not observed that in the living *Millepora* the soft parts of the hydrophyton appear to be retracted below the surface of the corallum when the zooids are in their retracted condition. It can, however, hardly be the case that any part of the corallum is directly exposed to the water. It is probably always covered everywhere by the superficial layer of the ectoderm, which, however, is in the recent condition so transparent as to escape observation. The calcareous tissue of the corallum must obviously be deposited by the ectoderm, with which alone it is in contact. It spreads by extension of the trabeculæ at the surface; and since there it is seen to be often in contact only with the cells of the superficial layer, it seems that these cells must have the power of producing it. The calcareous network undergoes thickening in the deeper parts of the hydrophyton, as must necessarily be the case, because of the formation of the tabulæ and lines of growth. In these parts no doubt the fusiform nucleated cells of the ectoderm are the instruments of the deposition of the carbonate of lime. No special calciferous tissue was observed, such as exists in *Heliopora cærulea.*

Beneath the layer of ectodermal cells in the vascular network of the hydrophyton lies a layer of apparently homogeneous membrane, which appears to form everywhere a wall to the vessels and canals. The cavities of the vascular network are lined by, and in many places nearly filled with, cellular elements of two kinds—pigmented cells and small transparent globules. The pigmented cells (Plate 3. fig. 17) closely resemble those of other hydroids. They are spheroidal in form, with transparent wall and contents composed of irregular granules, which are of a bright gamboge-yellow colour. It is these cells which give the bright yellow tinge to the tips of the living corallum. The cell-contents in these cells are frequently to be seen divided into two, each half having its own nucleus, or sometimes more rarely into four (Plate 3. fig. 17, *b*, *c*). The more superficial part of the vascular network of the hydrophyton is in most places almost crammed full of these pigmented cells, and they are abundantly present also within the somatic cavities of the zooids. They become less abundant towards the deeper parts of the living layer, and in certain of the deepest ramifications of the network are entirely absent, their place being taken by transparent globules. In some parts of the corallum large quantities of the pigmented cells are met with which are coloured dark brown instead of yellow. These belong probably to the older parts of the coral, which have in the living condition a brown appearance, it being only the growing tips which are bright yellow. Such, however, was not ascertained to be the case.

At the under surface of the living layer of the hydrophyton the vascular network has in connexion with it, or is prolonged into, a network of extremely transparent thin-walled vessels, many of which terminate in blind extremities, as shown in Plate 3. fig. 16, B. These vessels are distended with small exceedingly transparent and highly refractile globules, without any admixture of pigmented cells. These transparent globules are found scattered amongst the pigmented cells throughout the vessels of the hydrophyton, but occur in masses only as above described. No clue to the function of these transparent globules, nor explanation of their being thus agglomerated in the deeper parts of the living layer, was obtained; the masses of them probably point to a fatty degeneration of the effete deep regions of the network of the hydrophyton.

Thread-cells.

The thread-cells are of two kinds. They are shown, carefully drawn to measurement, in Plate 2. figs. 1 and 2. One kind is that which appears to be confined to Hydrozoa, and not to occur at all in Anthozoa, viz., that in which a bladder-like enlargement of the thread occurs at that part of it which is immediately next the mouth of the cell, the bladder being armed near its summit by three spines set in one whorl. The three spines in this form of thread-cell in *Millepora* are remarkably long, and directed at right angles to the axis of the thread, instead of recurved, as usual. These thread-cells vary very much in size. The one figured is one of the largest observed, being of about two thirds of the length of the ovoid thread-cells. The larger examples of these three-spined thread-cells are of comparatively rare occurrence, only a few being present in some of the tentacles, and being more commonly present in the tentacles of the mouthed zooids. The smaller thread-cells of this form have not more than $\frac{1}{5}$ of the length of the larger ones. They form the bulk of the spheroidal tips of the tentacles of the zooids; but both large and small thread-cells of this kind occur also in the hydrophyton. The larger ovoidal thread-cells are also such as occur in hydroids, but are not of so characteristic a form as the three-spined kind. They have been already figured by the late Professor AGASSIZ from *Millepora alcicornis*. These thread-cells never occur in the tentacles of the zooids, being confined to the hydrophyton, and being present in the greatest abundance in zonal masses around the bases of the zooids lying in the superficial layer of the ectoderm. Both these forms of thread-cells occur together in *Gemmaria implexa*, and with a similar distribution.

Both kinds of thread-cells occur, in all stages of development, in the ectoderm of the vascular network of the hydrophyton, extending in position to a considerable depth from the external surface of the coral (Plate 3. fig. 16).

The thread-cell appears to be developed out of the nucleus of the ectodermal cell, the ectodermal cell becoming much enlarged and forming a wide chamber, in which the process of development takes place. The ovoid nucleus becomes enlarged together with the cell, but not at all in the same proportion, the cell always appearing as a wide cavity around it. The nucleus, as it enlarges, has a rounded nucleolus developed at one end of it. The nucleolus has large granules developed within it, whilst the nucleus becomes finely granular. In the next stage one large coil of the thread appears in the nucleus. Nothing further could be made out from the hardened specimens as to the development of the thread-cells.

Most unfortunately no trace of generative organs could be detected in connexion with any of the zooids, neither in the *Millepora* from Tahiti nor in the other two species examined. These other two species have essentially the same structure as the Tahitian species, having mouthed and mouthless zooids. They have both of them closely similar thread-cells of both forms and with a similar distribution. Moreover the larger thread-cells have very nearly the same dimensions in all three species; they are a trifle smaller only in *Millepora alcicornis*. This latter species and the Zamboangan one differ from the Tahitian species mainly in not having their zooids grouped in distinct systems.

Vegetable Parasites.

I have already, in my paper on *Heliopora cærulea*, referred to the vegetable organisms infesting the corallum of *Millepora*. I have here only to add that the mycelium and spores are not confined to the calcareous structures, but occur also in abundance amongst the soft superficial tissues; and it appears probable that they become included within the calcareous tissue by the calcareous matter being deposited around them as the corallum is extended by growth. They are extremely abundant in the Tahitian species, and have a decided green colour*.

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* These organisms have, since this paper was written, been made the subject of two memoirs by Prof. P. MARTIN DUNCAN, F.R.S. See "On some *Thallophytes* parasitic within recent Madreporaria," Proc. Roy. Soc. no. 174, 1876, p. 238; "On some Unicellular Algæ parasitic within Silurian and Tertiary Corals, &c.," Quart. Journ. Geol. Soc., May 1876, p. 205.

Conclusions.

The discovery of the hydroid nature of *Millepora* was made by Prof. AGASSIZ as long ago as 1859[*]. AGASSIZ determined that there were two forms of zooids present in the coral—the one form broad, with from four to six knobbed tentacles; the other slender, with numerous similar tentacles disposed along their whole length. His conclusions with regard to *Millepora* have received only partial acceptation; they have been universally adopted in America and Germany. CLAUS and others have placed *Millepora* with Hydroids in their zoological systems. M. M.-EDWARDS, however[†], did not accept, at all events in 1860, Prof. AGASSIZ's results, and more recently Prof. ALLMAN has expressed his uncertainty in the matter on the ground of our ignorance of the generative system of the Milleporidæ[‡], and awaits the result of further researches. Although, most unfortunately, no evidence as to the structure of the generative system of *Millepora* was obtained by the present investigations, the results obtained yield, nevertheless, I think, convincing proofs that this interesting form of coral is a true Hydroid. The peculiar structure of the hydrophyton, the forms of the zooids, the absence of all trace of mesenteries, the apparent septa present in the tentacles, the presence of the thread-cells of the form peculiar to Hydrozoa, and in fact every item of histological structure point irresistibly to the same conclusion. Professor AGASSIZ considered the Millepores to be allied to the Hydractiniæ, and CLAUS remarks on their resemblance in some points to the Corynidæ. Both *Hydractinia* and *Podocoryne* resemble *Millepora* in having a hydrophyton which forms a continuous encrusting layer; and in essential structure the hydrophyton of these two genera seems closely to resemble that of *Millepora*. The genus *Podocoryne* (Sars) has a "hydrophyton consisting of a continuous adherent expansion formed by adnate inosculating canals, the deeper part, with its component canals, invested by a chitinous perisarc, while a layer of naked cœnosarc spreads over the free surface"[§]. In *Millepora* the canals are not adnate, being separated by the stout trabeculæ of calcareous matter which here take the place of the chitinous perisarc. The layer of naked cœnosarc on the surface is probably homologous with the layer in the hydrophyton of *Millepora* described in the present paper as the superficial layer of the ectoderm. The structure of the hydrophyton of *Hydractinia* is essentially similar to that of *Podocoryne*. Distinctive features in the hydrophyton of *Millepora* are the presence in it of calicular excavations into which the zooids are retracted, the presence of large main branching canals, and the formation of successive superposed layers of hydrophyton, and consequent formation of lines of growth and tabulæ in the calcareous skeleton. In having zooids of two kinds, mouthed and mouthless, the Millepores resemble *Hydractinia echinata*, which bears likewise alimentary (mouthed) and spiral (mouthless) zooids. In the form of the zooids, however, and shape and arrangement of the tentacles, and in the nature of the thread-cells*, *Millepora* seems to resemble such a form as *Gemmaria implexa*. The real affinities of *Millepora* amongst the Hydroids cannot, however, be determined until the mode of reproduction is discovered.

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[*] Bibl. Univ. de Genève, Arch. des Sciences, Mai 1859, p. 80.

[†] Hist. Nat. des Coralliaires, t. iii. p. 224.

[‡] ALLMAN, 'Gymnoblastic and Tubularian Hydroids,' p. 3.

[§] ALLMAN, *l. c.* vol. ii. p. 348.

It is a remarkable fact that the corallum of *Millepora* seems undoubtedly to be generated by the ectoderm. In Anthozoa this is not the case, the corallum being developed from a mesoderm, as appears certain in the latest accounts of the matter from M. LACAZE-DUTHIERS'S† researches on *Astroïdes calycularis*, and from those of KOWALEWSKY‡ on *Astræa* and on *Alcyonium digitatum*. In *Alcyonium* two elements are recognized by KOWALEWSKY as composing in the embryo the "intermediate layer" (mesoderm), viz. a homogeneous membrana propria, which lies internally and penetrates the mesenterial folds, and a peculiar thin layer of cells, which lies externally to this membrana propria. It is from this thin layer of cells that the gelatinous connective tissue, the spicules, and canal networks are formed. This special layer does not exist in other corals nor in *Cerianthus*.

The close resemblance in the histological structure of the coralla formed by animals so different as Alcyonaria (*Heliopora cœrulea*), Zoantharia, and Hydroida is a remarkable fact. The corallum of *Millepora* is distinguished from all other coralla by the presence within it of systems of canals branching in an arborescent manner. The presence of such structures in fossil corals will be sufficient evidence of the affinities of such corals with *Millepora* and the Hydroida. Now that *Heliopora* has been shown to be an Alcyonarian, there remains only one living genus, *Pliobothrus* (POURTALES), to form with *Millepora* the family Milleporidæ. Of *Pliobothrus* the soft structures have not been examined.

No representative of the genus *Millepora* appears to be known as existing in the fossil condition, at least none such is mentioned in QUENSTEDT'S 'Petrefactenkunde' or by M. MILNE-EDWARDS. A careful study of the internal structure of the various extinct corals which have been associated with *Heliopora* and *Millepora* amongst the Tabulata would show which of them have real affinities with *Millepora*. It would be well if the term Tabulata were dropped altogether, since it has reference to a structure common to certain Alcyonaria, Zoantharia, and Hydroida, and being not characteristic of any natural group only tends to confusion.

H.M.S. 'Challenger,' S.E. Pacific,
24th December, 1875.

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* *Note.*—It would seem that a classification and nomenclature of the various forms of thread-cells is much needed, since these forms appear to be of classificatory value in the Cœlenterata. Certain forms are peculiar to Hydroids, *e.g.*, others to Alcyonaria.

† LACAZE-DUTHIERS, H. DE. "Développement des polypes et de leur polypier," Comptes Rendus, 1873, t. lxxvii. (HOFFMAN und SCHWALBE, Jahresberichte, 1875.)

‡ KOWALEWSKY, A. "Untersuchungen über die Entwicklung der Cœlenteraten," Nachrichten der kaiserlichen Gesellschaft der Freunde der Naturerkenntniss, der Anthropologie und Ethnographie. Moskau, 1873. (*Ibid.*)

POSTSCRIPT, *added February* 22, 1877.

I have, since the above was written, discovered that the group of stony corals known as the Stylasteridæ is composed of true Hydroids, and have suggested that these should be grouped with the Milleporidæ in a special suborder, to be termed Hydrocorallinæ. (See Proc. R. Soc. no. 172, 1876, p. 99.)

DESCRIPTION OF PLATES ILLUSTRATING THE STRUCTURE OF MILLEPORA.

All the figures, with the single exception of fig. 5, Plate 2, represent the structures occurring in *Millepora* found at Tahiti, Society Islands, viz. *Millepora nodosa*.

PLATE 2.

Fig. 1. Ovoid thread-cell, confined in position to the bases of the zooids and general superficial layer, not occurring in the tentacles.

a. The cell unexpanded.

b. The cell with the thread fully projected.

The respective lengths of the various parts of the thread and cell are drawn exactly according to measurements.

Fig. 2. Three-barbed thread-cell, of the form peculiar to Hydrozoa, occurring in the tentacles of the zooids and also sparingly on the general surface of the coral.

a. The cell with its head protruded and thread partially projected.

b. The cell in the unexpanded condition. The figures are drawn exactly to measurements.

Fig. 3. View of a portion of the corallum, magnified 2 diameters.

Fig. 4. View of the surface of the corallum, magnified 80 diameters, showing one complete system of pores, composed of a central calicle, which contained in the recent condition a mouthed zooid, and eight surrounding smaller calicles of mouthless zooids.

Diameter of the central calicle =0·25 millim.
Largest diameter of the whole group =1·5 „

Drawn by Mr. J. J. WILD, Artist to the 'Challenger' Expedition.

Fig. 5. Enlarged view of a vertical section of the corallum.

C. Zooid-cavity or calicle.

B, B. Branches of the canal-system.

Fig. 6. Section of the corallum cut parallel to its outer surface, showing a portion of a system of zooids.

C. Calicle of mouthed zooid in horizontal section.

C′, C′. Calicles of mouthless zooids.

B′. Branch of a canal-system which communicates by means of a lateral offset with one of the zooid-cavities.

The dark linear bodies represented as embedded in the calcareous matter are cavities bored by parasitic vegetable organisms, the black dots being spore-cavities. In one of the calicles a branched mycelial thread is seen to cross over between the tubular cavities bored in the corallum.

Fig. 7 shows the appearance presented under transmitted light by a thin horizontal section of the corallum of a species of *Millepora* from Zamboangan, Philippines, in which the calicles and canal-systems have become filled with opake matter. The black streaks thus shown represent the ramifications of the canals of the hydrophyton within the tortuous canals in the substance of the corallum, and show their anastomoses with the calicular cavities.

Fig. 8. Portion of a fine section of the corallum, as seen under HARTNACH's objective No. 8, eyepiece No. 3.

The canals bored by the parasitic fungus are seen traversing the calcareous matter.

At (*a*) the fibro-crystalline elements of the hard tissue project in a series of points.

PLATE 3.

Fig. 9. View of a group of zooids in the expanded condition. In the centre is the short mouth-bearing zooid, provided with only four tentacles. Grouped around are seen five mouthless zooids. A sixth such zooid belonging to the group is omitted from the drawing for the sake of clearness.

Fig. 10. Vertical section of the decalcified living superficial layer. A mouthed zooid and a mouthless zooid are shown in the retracted condition. In the mouthed zooid one tentacle is omitted for the sake of clearness. The open spaces in the network of the hydrophyton are in the recent condition occupied by the calcareous network. The vessels are in places cut through, and have the pigmented endodermal cells exposed to view.

E. Superficial layer of the ectoderm.

Z. Mouthless zooid.

MZ. Mouthed zooid.

A. One of the spaces occupied by calcareous matter in the recent condition.

B. Secondary branch of one of the canals of the hydrophyton.

O. One of the cut openings into one of the vessels.

Fig. 11. View of a group of zooids as seen in the contracted condition. One mouthed and three of the surrounding mouthless zooids are shown. The mouthed zooid here has five tentacles. The surface of the decalcified coral is here

represented as seen when viewed from above with the microscope focused somewhat into the depths of the structure. A deep focus is necessary in order to reach the far-retracted zooids. The deeper reticulations of the hydrophyton are thus brought into view.

MZ. Mouthed zooid.
Z, Z. Mouthless zooids.

Fig. 12. View of the inferior surface of the superficial living layer, from a specimen decalcified in chromic acid and viewed by reflected light. The figure shows the ramifications of the canals and vessels of the hydrophyton and their connexions with the zooids of one complete group or system.

MZ. Under surface of mouthed zooid.
Z. One of the seven surrounding mouthless zooids.
C. Canal.
B, B. Branches of this canal.
B′, B′. Secondary branches, from which and from B B arises a complicated network of finer vessels.

Fig. 13. Enlarged view of a tentacle of a mouthless zooid.

K. Spherical head of the tentacle filled with thread-cells of various sizes.
E. Ectodermal layer.
RC. Ramified cells or nuclei of the endodermal layer.
M. Membranous layer.
P. Pigmented cells within the cavity of the tentacle.
C. Body-cavity continuous with that of the tentacle.

Fig. 14. Diagram showing the arrangement of the muscular fibres in a mouthed zooid. The longitudinal muscles are gathered into bundles, which pass outwards for insertion on to the radially disposed vessels of the hydrophyton. Other fibres, less densely placed, occupy the interspaces between these bundles.

O. Mouth of the zooid.
A, A. Radially disposed vascular offsets from the base of the zooid.
LM. Longitudinal muscular bundles.
CM. Circular muscular fibres.

Fig. 15. Transverse section of a mouthed zooid.

E. Ectodermal layer, containing thread-cells in various stages of development.
M. Membranous layer.
LM. Longitudinal muscular fibres seen in section as a series of dark points.
G. Gastric cells.

The narrow dark zone between the longitudinal muscles and membranous layer indicates a possibly existing circular muscular layer.

Fig. 16. Small portion of a vertical section of the hydrophyton, much enlarged, showing the histological structure of the vascular network. The vessels are seen cut open in almost their entire course. The walls of the deeper vessels are very thin, and these vessels are filled with transparent spherical globules. More superficially the walls of the vessels become thickened, and the cells composing their ectodermal layer are seen in several places to be in process of development into thread-cells. At the actual surface the cells of the ectoderm assume an elongate prismatic form. The vessels of the more superficial parts of the network are filled with the pigmented cells, mingled with transparent globules.

E. Superficial layer of the ectoderm.
M. Membranous layer of the hydrorhizal vessels.
C, C. Pigmented cells lying in the cavities of the vessels.
B. Transparent globules filling the deeper vessels.
T, T. Developing ovoid thread-cells.
T′. Developing thread-cells of the form peculiar to Hydrozoa.
S. Band of gelatinous tissue passing between the walls of two neighbouring vessels.
A, A. Spaces occupied in the recent condition by calcareous matter.
A′. Such a space in the superficial ectodermal layer.
O. Opening in a vessel cut at right angles to its course.

Fig. 17. Pigmented cells, of which the endoderm of the hydrorhizal vessels is mainly composed, and which are abundant also within the body-cavities of the zooids.

a, d, d. Examples of the cells, showing various forms and arrangements of the pigmented granules and vesicles which compose their contents.
c. Cell showing a division of its contents into two.
b. Cell showing a further division of its contents into four.

Fig. 16. Small portion of a vertical section of the integument, much enlarged, showing the histological structure of the vascular network. The vessels are seen cut open in almost their entire course. The walls of the deeper vessels are very thin, and these vessels are filled with transparent spherical globules. More superficially the walls of the vessels become thickened, and the cells composing their endothelial layer are seen in several places to be in process of development into blood-cells. At the actual surface the cells of the epidermal [illegible] an elongate [illegible] form. The vessels of the more superficial parts of the network are filled with the pigmented cells mingled with transparent globules.

S. Superficial layer of the ectoderm.

M. Membranous layer of the hypodermal vessels.

E, C. Pigmented cells lying in the meshes of the network.

H. Transparent globules filling the deeper vessels.

K, L. Developing round blood-cells.

N. [illegible]

R. Band of [illegible] fibres passing between the walls of two vessels.

G. [illegible] vessels.

P, A. [illegible] in the recent condition by refracting [illegible].

X. [illegible] in the superficial [illegible].

D. [illegible]

Fig. 17. [illegible] cells, [illegible] which [illegible] and which are [illegible] also [illegible] of the [illegible].

a to b. [illegible]

[illegible]

c. [illegible]

d. [illegible]

IV. *A Contribution to Terrestrial Magnetism; being the Record of Observations of the Magnetic Inclination, or Dip, made during the Voyage of H.M.S. 'Iron Duke' to China and Japan, &c.*, 1871–75. *By Vice-Admiral Sir* CHARLES SHADWELL, *K.C.B., F.R.S.*

Received and Read June 15, 1876.

IN the following paper I propose to place on record, and submit for the information of the Royal Society, the observations made by me for the determination of the magnetic inclination, or dip of the magnetic needle, during the recent voyages of H.M.S. 'Iron Duke' in the Eastern Seas, when visiting China, Japan, and places adjacent, between October 1871 and April 1875.

The instrument employed was a six-inch dip-circle by ROBINSON, furnished with two needles, and graduated to ten minutes, which was formerly the property of the late Capt. FRANCIS P. BLACKWOOD, R.N., and which was used by me during that officer's survey of the N.E. coast of Australia in H.M.S. 'Fly,' 1842–46.

This instrument was subsequently lent to me while I was in command of H.M.S. 'Highflyer,' employed on the China Station, 1857–59, and has since passed into my possession by gift from the son of my former Captain.

I have thus had the opportunity of repeating, with the same circle and needles, the observations made at various places in China and elsewhere after the lapse of some years, and have thus obtained data for the approximate deduction of the values of the secular changes in this element at some of the stations visited, which may perhaps give to these observations a greater value than they would otherwise possess.

Moreover, in some instances, I have not only had the opportunity of observing the dip at stations along the eastern coasts of Asia, but also of obtaining its value at some places in the chain of islands lying to the eastward from Labuan up to Yezo, which will thus afford the means, by aid of a graphic projection, of determining, approximately, the general direction of the "isoclinal lines" in those parts.

The observations were made generally in the manner described in the 'Admiralty Manual of Scientific Inquiry,' with every possible precaution to eliminate accidental errors of observation; the readings were taken both for the lower and upper ends of the needle, on the graduated circle, "face to face" and "face reversed," "face east" and "face west," "poles direct" and "poles reversed," three readings being taken in each position of the instrument, so that the final result for the dip for each needle is the mean of forty-eight readings; and the observations being repeated in like manner with the second needle, the "mean concluded dip" at each station is the mean result of the observations by the two needles.

In arranging the observations, after first recording in Table I. those taken on the voyages out and home, 1871 and 1875, to and from Singapore, I have given in Table II. those taken in the Eastern Seas, in order of the latitudes of the stations visited; but in all cases where the observations have been subsequently repeated at the same station I have noted all the results obtained at the same place in the Table in succession.

Finally, I have briefly compared the results of some of the observations here given with those made by me at the same stations at former periods, and have added such remarks as the comparisons suggested.

TABLE I.

No.	Date and time.	Station.	No. of needle.	Poles direct and reversed.	Mean dip.	Mean concluded dip.	Remarks.
1.	1871. Oct. 9. 6.30 A.M. 7.20 „	Malta, 35° 53′ N., 14° 31′ E.	1.	51° 50′·7 51 34·7	51° 42′·7	51° 42′·5 N.	Taken near Isola Point, south side of the Fortification Wall.
			2.	51 56·4 51 28·4	51 42·4		
2.	1875. Apr. 10. 5.40 A.M. 6.30 „	Same.	1.	51 38·8 51 40·8	51 39·5	51 38·9 N.	Ditto.
			2.	51 40·4 51 36·4	51 38·4		
3.	1875. Mar. 31. 3.10 P.M. 4.10 „	Port Said, Egypt, 31° 16′ N., 32° 19′ E.	1.	42 29·5 42 41	42 35·2	42 38·3 N.	Taken 200 yards N.W. of the Lighthouse.
			2.	42 44·1 42 38·8	42 41·4		
4.	1871. Oct. 24. 3. 0 P.M. 3.45 „	Suez, Egypt. 29° 57′ N., 32° 34′ E.	1.	40 52·6 40 56	40 54·3	40 54·5 N.	Taken at Observation Spot, North Pier of Dock.
			2.	40 1·2 40 48·2	40 54·8		
5.	1875. Mar. 23. 3.40 P.M. 4.30 „	Same.	1.	40 48·4 40 58·2	40 53·3	40 51·9 N.	Ditto.
			2.	40 55·5 40 45·8	40 50·6		
6.	1871. Nov. 7. 10.20 A.M. 11. 0 „	Aden, 12° 46′ N., 45° 5′ E.	1.	6 13·3 6 6·9	6 10·1	6 10 N.	Near a detached Bungalow at the Residency.
			2.	6 23·4 5 56·5	6 9·9		
7.	1875. Mar. 13. 6. 0 A.M. 6.50 „	Same.	1.	6 0·1 6 9·8	6 4·9	6 8·3 N.	Ditto.
			2.	6 16·4 6 7·2	6 11·8		
8.	1871. Nov. 30. 10.30 A.M. 11.30 „	Point de Galle, 6° 1′ N., 80° 13′ E.	1.	7 47 7 48·7	7 47·8	7 44 S.	In the Garden of the Queen's House.
			2.	7 35·9 7 44·8	7 40·3		
9.	1875. Feb. 23. 3. 0 P.M. 3.40 „	Same.	1.	8 0·7 7 39·9	7 50·3	7 46·8 S.	Same place, now become the property of Messrs. Clark, Spence, and Co.
			2.	7 35·8 7 50·9	7 43·3		
10.	1875. Feb. 17. 10.10 A.M. 11. 0 „	Trincomalee, Ceylon, 8° 34′ N., 81° 13′ E.	1.	1 59·9 1 49·2	1 54·5	1 52·4 S.	Under a Banyan tree, N.E. side of the Admiralty House.
			2.	1 44·1 1 56·6	1 50·3		

Table II.

No.	Date and time.	Station.	No. of needle.	Poles direct and reversed.	Mean dip.	Mean concluded dip.	Remarks.
11.	1874. Feb. 21. 7. 0 A.M. 7.55 „	Singapore, 1° 17′ N., 103° 51′ E.	1. 2.	13° 26′·5 13 9 13 0·6 13 25·9	13° 17′·7 13 13·3	13° 15′·5 S.	Taken in the Garden on N.E. side of Government House.
12.	1874. Feb. 23. 7.30 A.M. 8. 0 „	Same.	1. 2.	13 20·7 13 8·4 13 5·7 13 20·5	13 14·5 13 13·1	13 13·8 S.	Ditto.
13.	1875. Jan. 31. 6.45 A.M. 7.30 „	Same.	1. 2.	13 26·9 13 17·9 13 10·4 13 23·5	13 22·4 13 16·9	13 19·6 S.	Taken on the lawn in front of Government House.
14.	1872. Jan. 30. 11.30 A.M. 12.15 P.M.	Labuan, 5° 16′ N., 115° 15′ E.	1. 2.	2 11·9 2 3·8 2 7·6 2 11·8	2 7·8 2 9·7	2 8·7 S.	Taken under some trees on west side of Cemetery near the Flagstaff.
15.	1872. Jan. 4. 6.15 A.M. 7.10 „	Island of Penang, Straits of Malacca, 5° 25′ N., 100° 14′ E.	1. 2.	5 1·2 5 9·6 5 4·2 5 12·9	5 5·4 5 8·5	5 6·9 S.	Taken at the Compound at the Police Station, under a tree at the west end.
16.	1874. Feb. 1. 6.10 A.M. 7. 0 „	Same.	1. 2.	5 16·3 5 5·0 4 59·4 5 13·9	5 10·6 5 6·6	5 8·6 S.	Ditto.
17.	1874. Mar. 5. 6.30 A.M. 7.15 „	Saigon, Cochin China, 10° 47′ N., 106° 42′ E.	1. 2.	7 44·6 8 16·6 8 14·8 7 51·9	8 0·6 8 3·3	8 1·9 N.	On east side of Saigon river, opposite the town.
18.	1874. Jan. 14. 6.40 A.M. 7.30 „	Bankok, Siam, 13° 45′ N., 100° 29′ E.	1. 2.	12 56·9 13 16·7 13 21·4 12 56·9	13 6·8 13 9·1	13 7·9 N.	Taken in the Garden of the British Consulate.
19.	1872. Feb. 9. 6.50 A.M. 7.40 „	Manilla, Philippine Islands, 14° 36′ N., 120° 58′ E.	1. 2.	17 54·7 18 20·2 18 20·4 17 57·5	18 7·5 18 8·9	18 8·2 N.	Taken in the Garden of the British Consul's residence in the suburbs.
20.	1872. Apr. 12. 9.45 A.M. 10.30 „	Hong Kong, China, 22° 16′ N., 114° 10′ E.	1. 2.	32 16·4 32 24·5 32 23·4 32 7·7	32 20·4 32 15·5	32 17·9 N.	Taken at the Wellington Battery.
21.	1873. Apr. 1. 10. 0 A.M. 10.50 „	Same.	1. 2.	32 13·2 32 25·4 32 31·2 32 8·5	32 19·3 32 19·8	32 19·5 N.	Ditto.
22.	1874. Dec. 21. 9.50 A.M. 10.45 „	Same.	1. 2.	32 16·7 32 19·9 32 16·9 32 16	32 18·3 32 16·4	32 17·3 N.	Ditto.

TABLE II. (continued).

No.	Date and time.	Station.	No. of needle.	Poles direct and reversed.	Mean dip.	Mean concluded dip.	Remarks.
23.	1873. Apr. 24. 4.20 P.M. 5.10 „	Takau, Island of Formosa, 22° 37′ N., 120° 16′ E.	1.	32° 40′·4 32 53·8	32° 47′·1	32° 45′·8 N.	At Messrs. Brown's Garden, inside the Harbour of Takau.
			2.	32 53·8 32 35·2	32 44·5		
24.	1872. Mar. 13. 7.30 A.M. 8.20 „	Canton, China, 23° 8′ N., 113° 15′ E.	1.	34 16·2 33 22·9	33 49·5	33 39·9 N.	Taken in the Garden of the Tartar General's Yamen, the residence of Sir Brooke Robertson, C.B., H.M.'s Consul.
			2.	33 33·5 32 27·4	33 30·4		
25.	1872. Mar. 14. 7.20 A.M. 8. 5 „	Same.	1.	33 40·7 33 31·2	33 35·9	33 36·1 N.	Ditto.
			2.	33 47·6 33 25·2	33 36·4		
26.	1872. Apr. 20. 3. 0 P.M. 3.40 „	Swatow, China, 23° 21′ N., 116° 40′ E.	1.	33 58·9 34 16·9	34 7·9	34 9·9 N.	Taken in the Garden of the British Consulate.
			2.	34 22·3 34 1·8	34 12·0		
27.	1873. Apr. 22. 3. 0 P.M. 3.50 „	Pong Chow, Pescadores Islands, 23° 32′ N., 119° 34′ E.	1.	34 33·2 34 29·2	34 31·2	34 34·6 N.	Taken on the beach.
			2.	34 48·4 34 27·6	34 38		
28.	1872. Apr. 26. 4.15 P.M. 5. 0 „	Amoy, China, 24° 28′ N., 118° 4′ E.	1.	36 6·1 36 11·2	36 8·6	36 10·4 N.	Taken on the Island of Kulung-su, in the Garden of the British Consulate.
			2.	36 16 36 8·6	36 12·3		
29.	1873. Apr. 17. 10.10 A.M. 11. 0 „	Same.	1.	35 48·3 36 18·7	36 3·5	36 6·8 N.	Ditto.
			2.	36 18·1 36 2·1	36 10·1		
30.	1874. Apr. 25. 3.40 P.M. 4.20 „	Same.	1.	36 1·3 36 20·7	36 11	36 9·1 N.	Ditto.
			2.	36 20·1 35 54·4	36 7·2		
31.	1873. Apr. 29. 10. 0 A.M. 10.50 „	Kelung, Island of Formosa, China Sea, 25° 8′ N., 121° 45′ E.	1.	36 44·7 36 46·4	36 45·5	36 47 N.	Taken on the beach on the east side of the harbour.
			2.	36 54·3 36 42·8	36 48·5		
32.	1872. Dec. 18. 10.30 A.M. 11. 0 „	Foo Chow Foo, China, on the River Min, 26° 2′ N., 119° 18′ E.	1.	38 26·3 38 31·6	38 28·9	38 30·9 N.	Taken in the Garden of the British Consulate.
			2.	38 41·1 38 25	38 33		
33.	1872. Nov. 5. 3. 0 P.M. 3.40 „	Kiu Kiang, River Yangtoe, China, 29° 44′ N., 116° 8′ E.	1.	43 58 44 11·3	44 4·6	44 5·4 N.	In the Compound of the British Consulate.
			2.	44 16·9 43 55·6	44 6·3		
34.	1872. Dec. 12. 3. 0 P.M. 3.30 „	Ningpo, China, 29° 52′ N., 121° 34′ E.	1.	43 54·2 44 15·8	44 5	44 5·8 N.	In the Compound of the British Consulate.
			2.	44 13·4 44 0·1	44 6·7		

TABLE II. (continued).

No.	Date and time.	Station.	No. of needle.	Poles direct and reversed.	Mean dip.	Mean concluded dip.	Remarks.
35.	1872. Nov. 11. 4. 0 P.M. 4.30 „	Hankow, River Yangtse, China, 30° 33′ N., 114° 20′ E.	1.	45° 6′·1 45 32·2	45° 19′·1	45° 19′·5 N.	Taken in the Garden of the British Consulate.
			2.	45 24 45 15·9	45 19·9		
36.	1872. May 14. 9.30 A.M. 10.30 „	Shanghai, China, 31° 15′ N., 121° 29′ E.	1.	46 10·9 46 22·9	46 16·9	46 14·3 N.	Taken in the Grounds of the British Consulate, near the Flagstaff.
			2.	46 17·4 46 6	46 11·7		
37.	1873. May 30. 10. 0 A.M. 10.50 „	Same.	1.	46 17·6 46 25·8	46 21·7	46 19·1 N.	Ditto.
			2.	46 20·1 46 13·1	46 16·6		
38.	1874. May 18. 10.0 A.M. 11.0 „	Same.	1.	46 7·7 46 25·8	46 16·7	46 17·4 N.	Ditto.
			2.	46 26·7 46 9·6	46 18·2		
39.	1872. Nov. 16. 3. 0 P.M. 3.35 „	Woosung, mouth of the Shanghai River, 31° 23′ N., 121° 30′ E.	1.	45 59·8 46 4·7	46 2·3	46 4 N.	Taken in the Harbour-master's Garden near the Signal Staff.
			2.	46 11·6 45 59·8	46 5·7		
40.	1872. Nov. 1. 4. 0 P.M. 4.45 „	Nanking, River Yangtse, China, 32° 6′ N., 118° 44 E.	1.	47 5·6 47 19·4	47 12·5	47 13·6 N.	Taken on the shore. Right bank of the river.
			2.	47 23·1 47 6·5	47 14·8		
41.	1872. Oct. 31. 10.30 A.M. 11.15 „	Chin Kiang, River Yangtse, China, 32° 12′ N., 119° 21′ E.	1.	47 45·1 48 1·3	47 53·2	47 54·9 N.	Taken in a field below the British Consulate.
			2.	48 3·8 47 49·7	47 57·6		
42.	1872. May 31. 4.30 P.M. 5.20 „	Nagasaki, Island of Kiusu, Japan, 32° 44′ N., 129° 51′ E.	1.	46 23·7 46 45·5	46 34·6	46 32·4 N.	Taken in a small Cove west side of Bay, near the Dutch Factory.
			2.	46 39 46 21·6	46 30·3		
43.	1874. June 1. 10. 0 A.M. 10.45 „	Same.	1.	47 21·3 47 32·4	47 26·8	47 26·1 N.	At a place very near the above, not 100 yards distant.
			2.	47 29·1 47 21·8	47 25·4		
44.	1874. June 2. 4.20 P.M. 5. 5 „	Same.	1.	46 29·8 46 40·9	46 35·3	46 35·0 N.	The same place as on May 31, 1872.
			2.	46 38·6 46 30·8	46 34·7		
45.	1874. June 3. 10.20 A.M. 11. 0 „	Same.	1.	47 14 47 32·7	47 23·3	47 22·1 N.	The same place as on June 1, 1874.
			2.	47 28·9 47 13·1	47 21		
46.	1872. June 28. 6. 0 A.M. 6.45 „	Hiogo, Island of Niphon, Japan, 34° 42′ N., 135° 15′ E.	1.	48 27·1 48 40·6	48 33·8	48 32·6 N.	Taken in the rear of the German Union Club, Kobé.
			2.	48 43·1 48 19·8	48 31·4		

Table II. (continued).

No.	Date and time.	Station.	No. of needle.	Poles direct and reversed.	Mean dip.	Mean concluded dip.	Remarks.
47.	1873. Nov. 4. 3.20 P.M. 4. 0 „	Hiogo, Island of Niphon, Japan. 34° 42′ N., 135° 15′ E.	1.	48° 29′·5 48 37·6	48° 33′·5	48° 34′·9 N.	Taken in the rear of the German Union Club, Kobé.
			2.	48 39·8 48 32·9	48 36·3		
48.	1874. July 18. 5.45 A.M. 6.20 „	Same.	1.	48 37·5 48 41	48 39·2	48 37·2 N.	Ditto.
			2.	48 43·5 48 28·1	48 35·3		
49.	1872. July 13. 10. 0 A.M. 10.50 „	Yokohama, Island of Niphon, Japan, 35° 28′ N., 139° 40′ E.	1.	49 6·6 48 59·8	49 3·2	48 57·4 N.	Under a willow tree in front of Royal Marine Battalion Mess House, since become Naval Sick Quarters.
			2.	48 50·8 48 52·6	48 51·7		
50.	1873. July 2. 10.30 A.M. 11.20 „	Same.	1.	48 46·1 49 2·3	48 54·2	48 52·5 N.	Ditto.
			2.	49 0·3 48 41·3	48 50·8		
51.	1874. July 2. 10.10 A.M. 11. 0 „	Same.	1.	48 52·7 49 5·4	48 59	48 56·1 N.	Ditto.
			2.	49 1·9 48 44·8	48 53·3		
52.	1872. July 26. 6.30 A.M. 7.15 „	Yedo, Island of Niphon, Japan, 35° 40′ N., 139° 51′ E.	1.	49 15·7 49 19·6	49 17·6	49 14.6 N.	Taken in the Garden of the temporary British Legation.
			2.	49 18·6 49 4·8	49 11·7		
53.	1872. Sept. 11. 6. 0 A.M. 6.40 „	Chefoo, China, 37° 35′ N., 121° 22′ E.	1.	54 9·9 50 10·1	54 10	54 6·3 N.	Taken on the south side of the Bluff below the British Consulate.
			2.	54 9·4 53 55·9	54 2·6		
54.	1872. Sept. 14. 6. 0 A.M. 6.35 „	Tientsin, R. Peiho, China, 39° 9′ N., 117° 8′ E.	1.	56 15·2 56 24·6	56 19·9	56 20·3 N.	In the Compound of the British Consulate.
			2.	56 22·5 56 18·9	56 20·7		
55.	1873. Oct. 1. 2.15 P.M. 3. 0 „	Yamada, Island of Niphon, Japan, 39° 27′ N., 141° 59′ E.	1.	53 38·7 54 0·2	53 49·4	53 49·4 N.	On the East side of Oosima Island.
			2.	53 59·2 53 39·6	53 49·4		
56.	1872. Oct. 8. 6.30 A.M. 7.30 „	Yinkoa, the Port of New Chang, China, 40° 43′ N., 122° 15′ E.	1.	57 30·2 57 46·9	57 38·5	57 40 N.	In the Compound of the United States Consulate.
			2.	57 43·2 57 39·8	57 41·5		
57.	1872. Aug. 13. 10. 0 A.M. 10.50 „	Hakodaté, Island of Yezo, Japan, 41° 47′ N., 140° 45′ E.	1.	55 21·4 55 35·8	55 28·6	55 32·9 N.	Taken in the Garden of the British Consulate.
			2.	55 44·6 55 30	55 37·3		
58.	1873. Sept. 24. 10.30 A.M. 11.10 „	Same.	1.	55 37·2 55 48·1	55 42·6	55 39 N.	Ditto.
			2.	55 42 55 29	55 35·5		

TABLE II. (continued).

No.	Date and time.	Station.	No. of needle.	Poles direct and reversed.	Mean dip.	Mean concluded dip.	Remarks.
59.	1874. Sept. 5. 2.45 P.M. 3.30 „	Island of Yezo, Japan, 41° 4′ N., 140° 45′ E.	1.	55° 35′·7	55° 37′·5	55° 38′·5 N.	Taken in the Garden of the British Consulate.
				55 39·4			
			2.	55 42·4	55 39·6		
				55 36·8			
60.	1873. Aug. 2. 5.45 A.M. 6.20 „	Possiette Bay, Manchuria, 42° 38′ N., 130° 45′ E.	1.	58 51·3	58 54·2	58 53·4 N.	Taken at Observation-Spot, Tchurkhoda.
				58 57			
			2.	59 3·7	58 52·7		
				58 41·7			
61.	1873. Aug. 9. 5.45 A.M. 6.20 „	Vladivostock, Coast of Manchuria, 43° 7′ N., 131° 53′ E.	1.	58 48·5	58 48·3	58 45·4 N.	Taken at S.E. corner of the Commandant Garden.
				58 48·1			
			2.	58 47·8	58 42·5		
				58 37·2			
62.	1874. Aug. 18. 10 A.M.	Same.	1.	58 49·8	58 46·8	58 44 N.	Observation on Aug. 18, interrupted by rain, completed on Aug. 19.
				58 43·9			
	Aug. 19. 3.20 P.M.	Same.	2.	58 40·4	58 41·2		
				58 42·1			
63.	1873. Aug. 18. 5.45 A.M. 6.30 „	St. Vladimir Bay, Manchuria, 43° 54′ N., 135° 27′ E.	1.	58 56·3	58 57·9	59 0 N.	Taken on N.E. side of Low Point.
				58 59·6			
			2.	59 6·9	59 2·2		
				58 57·4			
64.	1873. Aug. 25. 5.35 A.M. 6.20 „	Port Imperial, Manchuria, 49° 2′ N., 140° 19′ E.	1.	63 17·8	63 24·4	63 25·9 N.	Near the Pier, in Pallas Bay.
				63 31			
			2.	63 32	63 29·4		
				63 22·9			
65.	1873. Aug. 28. 5.15 A.M. 6.35 „	Castries Bay, Manchuria, 51° 28′ N., 140° 51′ E.	1.	65 16	65 12·2	65 8·2 N.	Taken on Observatory Island.
				65 8·4			
			2.	65 8·2	65 4·3		
				65 0·5			
66.	1873. Aug. 31. 5.50 A.M. 6.30 „	Nicolaevsk, Siberia, R. Amur, 53° 8′ N., 140° 45′ E.	1.	66 23·7	66 28·4	66 29 N.	Taken on N. side of the River, near the Naval Factory.
				66 33·1			
			2.	66 33	66 29·7		
				66 26·4			

Remarks on the preceding Observations.

1. In comparing the results obtained for the value of the Magnetic Dip at any station with those formerly determined at the same place, I assume, in default of any evidence to the contrary, that all the observations are equally trustworthy; also that where the mean has been taken between two or more observations at the same place, made at different times, that the mean value of the dip so deduced corresponds to the mean of the times of observation considered as a mean epoch. The following results, however, are offered only as approximations.

2. *Observations at Malta on Dec.* 20, 1861, *or* 1861·97.—At the same spot of observation, with the same instrument and needles, the dip was found to be 52° 17′·3 N.

Reducing the observations at Malta recorded in Table I. to the same epoch, we have, 1873·52, mean dip 51° 40′·7. Consequently we have

	1861·97		Dip 52° 17′·3 N.
	1873·52		51° 40′·7 N.
Interval . . .	11·55	Change . . .	36′·6

And $\frac{36\cdot6}{11\cdot55}=3'\cdot16$, mean annual change. North dip decreasing.

3. *Observations at Singapore on April* 25, 1857, *or* 1857·31.—The mean concluded dip at Singapore was 13° 21′·2 S. The observations at Singapore recorded in Table II., reduced to the mean epoch 1874·45, give as the mean dip 13° 16′·3 S. Hence

	1857·31		Dip 13° 21′·2
	1874·45		13° 16′·3
Interval . . .	17·14	Change . . .	4′·9

And $\frac{4\cdot9}{17\cdot14}=0'\cdot29$ nearly. South dip decreasing.

4. *Observations at Hong Kong.*—Former observations at Hong Kong on May 26, 1857, Feb. 22, 1858, and Dec. 12, 1859, gave, as the values of the dip at the Wellington Battery at Hong Kong, 31° 26′·0, 31° 25′·3, and 31° 28′·5 respectively.

Hence, reducing to the mean epoch, we have 1858·49, dip=31° 26′·6 N.

Similarly the observations noted in Table II. give 1873·50, dip 32° 18′·2 N.

Interval 15 years. Change 51′·6.

And $\frac{51\cdot6}{15}=3'\cdot44$, mean annual change. North dip increasing.

5. *Observations at Canton on Jan.* 19, 1858, *or* 1858·05.—The dip at Canton was 32° 34′·5.

Reducing the observations recorded in Table II. to the mean epoch, we have 1872·20, dip 33° 38′ N. Interval 14·15 years. Change 63′·5.

And $\frac{63\cdot5}{14\cdot15}=4'\cdot49$, mean annual change. North dip increasing.

6. *Observations at Shanghai.*—Former observations at Shanghai on June 10, 1858, and Nov. 24, 1858, gave as the values of the dip 45° 18′·3 and 45° 19′·4 respectively.

Hence, reducing to the mean epoch, we have 1858·67, dip 45° 18′·8 N.

Similarly the observations recorded in Table II. give 1873·38, dip 46° 16′·9 N.

Interval 14·72 years. Change 58′·1.

And $\frac{58\cdot1}{14\cdot72}=3'\cdot95$. North dip increasing.

7. *Observations at Woosung.*—Former observations at Woosung on Nov. 28, 1858, gave the dip 45° 2′·5 N.

On Nov. 16, 1872, the dip was 46° 4′. Hence in 13·97 years the total change by these observations has been 61′·5.

And $\frac{61\cdot5}{13\cdot97}=4'\cdot40$, the mean annual change. North dip increasing.

8. *Observations at Ningpo.*—At Ningpo, on Oct. 6, 1858, the dip was found to be 43° 3′·5 N., and the observation being repeated on April 11, 1859, gave 43° 7′·9 N. Hence, reducing to the same epoch, we have 1859·02, mean dip 43° 5′·7 N.

Comparing this with the observation recorded in Table II., No. 34, 1872·94, 44° 5′·8 N., we find the change in 13·93 years to have been 60′·1.

And $\frac{60\cdot 1}{13.93}=4'\cdot 31$, the mean annual change. North dip increasing.

9. *Observations at Nagasaki, Japan.*—The observations at Nagasaki recorded in Table II., Nos. 42–45, present some singular discordances, which I can only account for on the supposition of some local disturbances affecting the observations. The places of observation were about 8 feet above the level of the sea, on a ridge of land under high cliffs. I was under the impression at the time that the spot of observation of No. 42 was as nearly as possible, as far as I was able to recognize the position, in the same place as where I had previously observed on Sept. 12, 1859, when I obtained 45° 53′·7 N. as the then value of the dip.

The anomalous results obtained on June 1, 1874, at a position not 100 yards distant, but which I thought might be slightly more convenient, induced me to repeat the observation at each of the selected stations, on the two following days, the results confirming those previously found at each place, showing that the discordances were real, and not apparent only, depending on some accidental errors of observation.

Under these circumstances I cannot treat the observations in the same manner as I have done those at other stations, and the attempt to indicate approximately the value of the annual change must be abandoned.

The inference I would draw from this experience is that it is not advisable to observe near high masses of land, which may possibly, by their attraction, influence the indications of the needles*.

10. *Observations at Yokohama, Japan.*—On Oct. 25, 1859, or 1859·81, the dip taken at Kanogowa, which was the then site of the British Consulate, was 48° 8′·7 N.

Kanogowa has since been given up as a settlement for the foreign merchants, and Yokohama has been selected instead.

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* [With reference to the discrepancies noticed at Nagasaki between the results at two stations not far distant, having reexamined my notes, and having also referred to the gentlemen who assisted me on the occasions referred to, I wish to add the following remarks:—

The spot of observation selected on May 31, 1872, and on June 2nd, 1874, for observations Nos. 42 & 44, was on a piece of level ground near the edge of the water of the harbour, behind it was a low cliff about 8 feet high and about 15 yards distant, beyond which a steep wooded slope rose upwards to the summit of the hills in the rear.

At the second station, where observations 43 and 45 were taken on June 1st and 3rd, 1874, the hill had been cut away so as to obtain a level space for the formation of a dock. I estimate the distance from the cliff as about 50 yards, the height of the cliff about 40 feet, the land in its rear sloping upwards, and forming hills from 200 to 300 feet high.

The two stations were about 90 or 100 yards distant, and their height above the sea-level about 8 feet.—C. S., June 3, 1877.]

Yokohama is about three miles from Kanogowa in an E.S.E. direction. There can be no objection, for the purpose in hand, to compare the observations made at the two places.

Reducing the Yokohama observations, Nos. 49–51, recorded in Table II., to a mean epoch, we have 1873·51, mean dip 48° 55′·3 N.

Hence in 13·69 years the dip has changed by these observations 46′·6.

And $\frac{46 \cdot 6}{13 \cdot 69}=3' \cdot 40$, mean annual change. North dip increasing.

11. *Observations at Yedo.*—On Sept. 22, 1859, observations taken at the Temple of "To-sen-dse," then the temporary residence of the British Embassy, gave as the value of the magnetic dip 48° 21′·8 N.

In 1872 the embassy was temporarily located at another place, a little east of the former position, but nearly on the same parallel. Here we had (Table II. No. 52), 1872·56, dip 49° 14′·6 N.

Hence in 13·84 years the dip has changed 52′·8.

And $\frac{52 \cdot 8}{13 \cdot 84}=3' \cdot 82$, mean annual change. North dip increasing.

12. *Observations at Hakodaté, Island of Yezo, Japan.*—Former observations made at Hakodaté on Oct. 13, 1859, gave as the value of the magnetic dip, 1859·78, dip= 55° 23′·3 N.

The observations given in Table II., Nos. 57–59, reduced to a mean epoch, give 1873·67, mean dip=55° 36′·8.

Hence in 13·89 years the dip has changed 13′·5.

And $\frac{13 \cdot 5}{13 \cdot 89}=0' \cdot 97$, mean annual change. North dip increasing.

13. *Observations at St. Vladimir Bay, Coast of Manchuria.*—Observations taken on Sept. 2, 1859, or 1859·66, gave as the value of the dip 58° 37′·8 N.

Comparing this with No. 63, given in Table II., 1873·63, 59° 0′ N., it appears that the dip had changed 22′·2 in 13·96 years.

And $\frac{22 \cdot 2}{13 \cdot 96}=1 \cdot 59$, the mean annual change. North dip increasing.

Note A.—The observation taken at Malta in December 1861, hereinbefore referred to, was communicated to the Admiralty in MSS. in 1862, and is doubtless in the records of the Hydrographic Office.

This observation is referred to in Sir E. Sabine's Contrib. Terrestrial Magnetism, No. xiv., Phil. Trans. 1875, part i. p. 190.

Note B.—The observation at St. Vladimir Bay in 1859 is referred to in Sir E. Sabine's Paper, Contrib. Terr. Mag. No. xiii., Phil. Trans. 1872, p. 362.

Note C.—All the observations made by me, 1857–59, above referred to were forwarded to the Admiralty in MSS., and are no doubt deposited among the records of the Hydrographic Office.

In so far as I am aware they have never yet been published, with the exceptions above named (Notes A & B).

POSTSCRIPT.—Feb. 8, 1877.

The magnetic dip was determined at Kew by Mr. WHIPPLE, with the instrument and needles employed in the preceding observations, with the following results:—

			°	′
January 1877.	Mean dip.	Needle No. 1 . . .	67	57·87
		„ No. 2 . . .	68	1·11
	Mean dip by Kew Standard		67	46·59
Corrections.	January 1877.	Needle No. 1 . . .		11·28
		„ No. 2 . . .		14·52

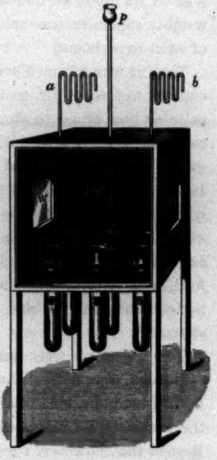
V. *Further Researches on the Deportment and Vital Persistence of Putrefactive and Infective Organisms from a Physical point of View.* *By* JOHN TYNDALL, *D.C.L., LL.D., F.R.S.*

Received May 14,—Read May 17, 1877.

§ 1. *Introduction.*

ON the 13th of January, 1876, I had the honour of submitting to the Royal Society some account of an investigation in which the power of atmospheric air to produce life in organic infusions and its power to scatter light were shown to go hand in hand. The "scattering" was proved to be due, not to the air itself, but to foreign matter suspended in the air. It was moreover proved that air placed under proper conditions went through a process of self-purification, and that, when this purification was visibly complete, the power to scatter light and to generate life disappeared together.

Fig. 1.



The form of the experiments here referred to was, it will be remembered, as follows:—Wooden chambers were constructed with glass fronts, side windows, and back doors. Through the bottoms of the chambers test-tubes passed air-tight, their open ends, for about one fifth of the length of the tubes, being within the chambers. Provision was made for a connexion through sinuous channels between the outer and the inner air. The chambers being closely sealed, were permitted to remain undisturbed for a few days. The floating matter of the internal air gradually subsided, until at length an intensely luminous beam failed to show its track within the chamber. Then, and not till then, were the infusions introduced, by means of a pipette passing through the top of the chamber. After their introduction, they were boiled in an oil- or brine-bath[*] for five minutes, and afterwards placed permanently in a warm room.

The annexed woodcut, taken from the 'Proceedings' of the Royal Institution, shows a chamber with its six test-tubes, its side windows *w w*, its pipette C, and its bent tubes *a b*, which connect the air of the chamber with the external air.

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[*] From the fact of their being boiled in oil or brine, Prof. COHN has inadvertently inferred that the infusions themselves were raised above their boiling-points. The tubes being open, the temperature of ebullition is of course independent of the source which provokes it.

In upwards of fifty chambers thus constructed, many of them used more than once, it was, without exception, proved that the sterilized infusion in contact with air shown to be self-cleansed by the luminous beam remained sterile. Never, in a single unexplained instance, did such an infusion show any signs of life. That the observed sterility was not due to any lack of nutritive power in the infusion, was proved by opening the back door and permitting the uncleansed air to enter the chamber. The contact of the floating matter with the infusions was invariably followed by the development of life. Numerous examples of these results were placed before the Fellows of the Royal Society at their Meeting on the 13th of January, 1876.

Prior to the date here referred to, great public interest had been excited, and, I may add, considerable scientific uncertainty had been produced in reference to this subject, both in England and America, by the writings of Dr. BASTIAN. These writings consisted, in part, of theoretic considerations and reflections, not new, but sometimes very ably stated, based on the general doctrine of Evolution, and, in part, of very pungent criticisms of those who, though believers in Evolution, declined to accept the writer's programme of its operations[*]. Passing over both theory and criticism, I thought it wise to fix upon certain well-defined statements of fact which lay at the basis of the weighty superstructure raised by their author, and to bring these statements to the test of strict experiment.

Thus it was affirmed "that boiled turnip- or hay-infusions exposed to ordinary air, exposed to filtered air, to calcined air, or shut off altogether from contact with air, are more or less prone to swarm with *Bacteria* and *Vibriones* in the course of from two to six days"[†]. I resorted accordingly to filtered air, calcined air, and to infusions withdrawn from air, but failed to discover the alleged "proneness" to run into living forms. It had also been affirmed that infusions of muscle, kidney, or liver, placed "in a flask whose neck is drawn out and narrowed in the blowpipe flame, boiled, sealed during ebullition, and kept in a warm place, swarmed after a variable time with *Bacteria* and allied organisms"[‡]. I resorted to such flasks, employing infusions of fish, flesh, fowl, and viscera, and on the 13th of January was able to place before the Royal Society one hundred and thirty of them, every one of which negatived the foregoing statement.

Two objections were subsequently urged against these results. The infusions, it was contended, were not sufficiently concentrated, nor were the temperatures sufficiently high. Both these objections were met by the statement that forty-eight hours' exposure under the same circumstances to common air sufficed to fill these same infusions with life. Beyond this, however, I was able to show that the temperatures employed by me were exactly those which had previously been found most effectual by the writer who urged the objection. Other temperatures, higher than any previously employed, were at the same time said to ensure spontaneous generation. I exposed my infusions to these newly discovered efficient temperatures, but found that they remained as barren as before.

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[*] See 'Evolution, or the Origin of Life,' pp. 168, 169.

[†] Evolution, p. 94.

[‡] Transactions of the Pathological Society, 1875, p. 272.

With regard, moreover, to the question of concentration, it was shown that, owing to their gradual vaporization, the infusions used by me were probably unequalled in strength by those employed by any previous investigator. Some of these infusions remain with me to the present hour. Concentrated by twelve months' slow evaporation, and reduced to one fifth of their primitive volume, they still exhibit the purity of distilled water.

These results have been published in the Philosophical Transactions, and Dr. BASTIAN has made no attempt to invalidate them. They prove beyond a doubt that in the atmospheric conditions existing in the laboratory of the Royal Institution during the autumn, winter, and spring of 1875–76, five minutes' boiling sufficed to sterilize organic liquids of the most diverse kinds. Among these may be mentioned urine in its natural condition, infusions of mutton, beef, pork, hay, turnip, haddock, sole, salmon, cod-fish, turbot, mullet, herring, eel, oyster, whiting, liver, kidney, hare, rabbit, barn-door fowl, grouse, and pheasant. Once properly sterilized, and protected afterwards from the floating matter of the air, not one of these putrescible infusions ever manifested the power of generating by its own inherent energy putrefactive organisms of any kind.

§ 2. *Experiments of* PASTEUR, ROBERTS, *and* COHN.

During the investigation just referred to I confined myself for the most part to animal and vegetable juices in their natural condition—that is to say, extracted by distilled water, and not rendered artificially acid, neutral, or alkaline. I had occasion, however, to repeat among others some of the very remarkable experiments on superneutralized hay-infusions described by Dr. WM. ROBERTS in his excellent paper in the Philosophical Transactions for 1874. These experiments I could not corroborate; for while in his hands such infusions sometimes required three hours' boiling to sterilize them, in mine they behaved like other infusions, and were sterilized in five minutes.

In the abstract of the investigation communicated to the Royal Society on the 13th of January, 1876, I mentioned this discrepancy, and pointed out its possible cause[*]. But the largeness of the question, which had been long previously raised by M. PASTEUR, and the limitation of my time led me to postpone it. This postponement is mentioned at the conclusion of my paper in the Philosophical Transactions, where the discrepancy referred to is not at all discussed.

In his celebrated paper, "Sur les corpuscules organisés qui existent dans l'Atmosphère," published fifteen years ago[†], M. PASTEUR first announced that while acid infusions had their germinal life destroyed by a temperature of 100° C., a temperature over 100° was needed to produce the same effect in alkaline infusions. In his 'Études sur la Bière,' published in the early part of 1876, he repeats and illustrates this statement.

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[*] Roy. Soc. Proc. vol. xxiv. p. 178.

[†] Annales de Chimie, 1862, vol. lxiv.

Vinegar he finds has the organisms which decompose it destroyed by a temperature of 50° C. Wine is rendered unchangeable by a slightly higher temperature. Beer-wort without hops requires a temperature of 90° C. to sterilize it, and milk a temperature of 110°. Fresh urine has its organisms destroyed at a temperature of 100°, while a higher temperature is needed when the urine has been neutralized by carbonate of lime[*]. The resistance of alkalized urine to sterilization is therefore by no means a new announcement[†].

On my return from Switzerland last autumn the experiments on alkalized hay-infusions were resumed; and soon afterwards Professor Cohn, of Breslau, so highly distinguished by his researches on *Bacteria*, placed in my hands a memoir[‡] which rendered it doubly incumbent on me to examine more strictly the grounds of my dissidence from Dr. Roberts. Professor Cohn is emphatic in his corroboration of Dr. Roberts[§], having found, during a long and varied series of experiments with hay-infusions of divers kinds, that when the period of boiling did not exceed fifteen minutes organisms invariably appeared in the infusions afterwards. Sixty, eighty, and even one hundred and twenty minutes' boiling were found in some cases insufficient to sterilize the infusions. One marked difference, however, exists between Dr. Roberts and Professor Cohn. The former found five minutes' boiling sufficient to sterilize unneutralized hay-infusion, but one, two, and even three hours' boiling insufficient to sterilize superneutralized hay-infusion; while the latter noticed no difference of this kind, but found acid and neutral infusions equally resistant[||].

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[*] Études sur la Bière, p. 34.

[†] With regard to the different action of acid and alkaline liquids, I put the subject purposely aside with the view to its full investigation as soon as the first instalment of these researches had been published. I could find no adequate explanation of the alleged fact that germs are killed in an acid liquid, while they survive in an alkaline one of the same temperature; nor could the well-merited respect that I feel for M. Pasteur cause me to accept his explanation of the fact without further inquiry on my own account. In due time, therefore, I resolved to examine the question. Various experiments and explanatory views regarding it are recorded in the following pages. It is perhaps worth mentioning that in his communication to the Academy of Sciences Dr. Bastian so interprets my last paper (Phil. Trans. 1876, p. 57) as to make me say that which I had neither the warrant nor the wish to say—namely, that germs are killed in alkaline liquids of all kinds by one or two minutes' exposure to a temperature of 212° F.

[‡] Beiträge zur Biologie der Pflanzen, July 1876.

[§] Professor Cohn gently censures me for taking exception to the cotton-wool plug, seeing that cotton-wool, even in my own experiments, has always proved a trustworthy filter. I did not, however, object to it as a filter, but on grounds which have in part, at all events, commended themselves to Professor Cohn himself. With reference to the method of Dr. Roberts he writes thus:—"The defect of this method consists in the difficulty of protecting the cotton-wool from accidental wetting by the infusion. The steam, moreover, which rises from the liquid penetrates the cotton-wool, and, through its partial condensation in the neck of the bulb, might readily charge itself with germs."

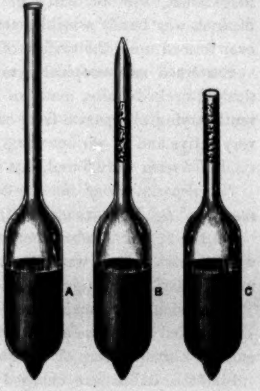
[||] "Ein constanter Unterschied in der Zeitdauer zwischen sauren und neutralen Aufgüssen, wie ihn Roberts gefunden, trat in unseren Versuchen nicht hervor" (p. 259).

§ 3. *Hay-infusions. Preliminary Experiments with Pipette-bulbs.*

I have now the honour to submit to the Royal Society an investigation which embraces among others the points here referred to, and which has proved far more difficult and laborious than I expected it would be. On the 27th of September, 1876, a quantity of chopped hay was digested for three hours and a half in distilled water maintained at a temperature of 120° Fahr. The infusion was afterwards poured off, and its specific gravity reduced to the exact figure given by Dr. Roberts, viz. 1006. It was then filtered and slightly superneutralized. Precipitation occurred on the addition of the potash, and the infusion was boiled for five minutes to render the precipitation complete. It was then refiltered, and introduced into a series of bulbs of the same size and character as those described by Dr. Roberts, and called by him "plugged bulbs"[*].

Each bulb was a cylinder about four inches high and upwards of an inch wide, with a long neck attached to it[†], as shown at A, fig. 2. Two thirds of the cylinder were occupied by the infusion. After the introduction of the latter, the neck of the bulb was plugged with cotton-wool, and hermetically sealed above the plug, as at B, fig. 2. The bulbs were afterwards plunged in water deep enough to cover their necks, which was gradually raised to the boiling-point, and maintained at the boiling temperature for ten minutes. They were then removed and permitted to cool; after which the sealed end of each neck was broken off by means of a file, its subsequent appearance being shown at C, fig. 2. The bulbs, protected by the cotton-wool plugs in the neck above them, were then exposed to a tolerably uniform temperature of about 90° Fahr.

Fig. 2.



At the same time two similar bulbs, charged with the same infusion, had their necks bent downwards, as in fig. 3 (p. 6), the inclined portion being plugged, so that no impurity could fall into the liquid from the cotton-wool. These two bulbs were boiled for five minutes in an oil-bath, and plugged while boiling with cotton-wool. They were then sealed behind the plugs and permitted to cool, their sealed ends being broken off afterwards.

On the 30th of September the infusion in all the straight-necked bulbs was turbid, while in the two bent-necked ones it was perfectly clear. On the 2nd of October the turbidity of the straight-necked bulbs had increased, while a fatty scum had formed on the surface of each. The two others were at the same time slightly but distinctly turbid.

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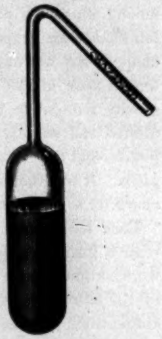
[*] Phil. Trans. vol. clxiv. p. 460.

[†] I have called them pipette-bulbs because they are formed by hermetically sealing one shank of a pipette, close to the bulb, leaving the other shank open for the introduction of the infusions. German pipettes, on account of their cheapness, were at first commonly used; but in cases of long-continued boiling, explosions were so frequent that bulbs of English glass of specially resistant quality were resorted to.

Fig. 3.



My inference from this experiment was that in neither the straight-necked nor the bent-necked bulbs had the germs been killed by the boiling, but that they were more damaged in the former than in the latter.

It is here to be noted that a quantity of air, with its associated floating matter, was imprisoned above the infusion in every straight-necked bulb; that in the case of the two bent-necked bulbs this air had been in part displaced by steam, the air which entered on cooling being sifted by the cotton-wool plugs. To this difference of treatment is to be attributed the observed difference of deportment. Unlike the thick cloudiness of their neighbours, the turbidity of the bent-necked bulbs, though distinct, was barely sensible, and in none of them was any scum ever formed upon the surface of the infusion.

Examined microscopically, numerous Vibrios were found in the infusions of the straight-necked bulbs, many of them broken at the centre, with the two halves apparently trying to separate from each other. There were also numerous smaller *Bacteria*, very active and of various lengths. In the bent-necked bulbs a number of exceedingly small *Bacteria* were found, but no Vibrios.

The deportment of the hay-infusion employed in these experiments corroborates the results of Dr. Roberts *and Professor* Cohn.

On the 2nd of October another infusion of hay was prepared, and, after neutralization with caustic potash, was introduced into six pipette-bulbs with straight necks. The necks, being first plugged with cotton-wool, were afterwards sealed by the blowpipe. The infusions were maintained for ten minutes at the temperature of boiling water. Their sealed ends were afterwards broken off, and they were subjected, like the former ones, to a temperature of 90° Fahr.

Six other bulbs were charged at the same time with the same infusion; but instead of being hermetically sealed, they were placed in an oil-bath, and boiled there for five minutes. Before the ebullition ceased, the neck of each was stopped with a plug of cotton-wool.

Up to October 6th all the bulbs continued clear. On the 6th one bulb of the series last described became turbid, lighter in colour than its neighbours, and covered with a fatty scum. On the 7th one tube of the first series (boiled after the fashion of Roberts for ten minutes) also became turbid and exhibited the same fatty scum. The remaining ten bulbs maintained permanently their deep brown-sherry colour, their high transparency, and their perfect freedom from Bacterial life. They are still clear, though seven months have elapsed since their preparation.

In the great majority of these experiments the deportment of alkalized hay-infusion contradicts that observed by Dr. ROBERTS *and Professor* COHN.

Six other pipette-bulbs, with their necks so bent and plugged with cotton-wool and asbestos that no impurity falling from the plug could reach the infusion, were also charged on the 2nd of October. Three of the bulbs, with their necks hermetically sealed, were maintained for ten minutes at the temperature of boiling water, the sealed ends being afterwards broken off. The three other bulbs were boiled in an oil-bath, and had their necks plugged before ebullition ceased. All six bulbs have remained perfectly transparent up to the present time.

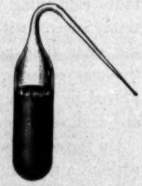
Here, again, we have discordance between my results and those of Dr. ROBERTS *and Professor* COHN.

But on the 6th of October another infusion was prepared and neutralized, exactly in the same fashion as before. Five pipette-bulbs were charged with it; they were hermetically sealed and immersed for ten minutes in boiling water. The sealed ends were afterwards broken off, and the bulbs exposed to a temperature of 90° Fahr. On the morning of the 8th of October (that is to say, two days after their preparation) the infusion in every one of the bulbs was turbid and covered with scum.

Here once more we have perfect harmony between my results and those of Dr. ROBERTS *and Professor* COHN.

Reverting to the 2nd of October, fourteen of our ordinary small retort-flasks with bent necks (shown in fig. 4) were then charged with the neutralized hay-infusion. They were boiled for three minutes, and hermetically sealed whilst boiling. Some days afterwards one tube of the entire number was observed to have become lighter in colour and sensibly cloudy; but thirteen out of the fourteen remained unchanged in colour, brightly transparent, and entirely free from life.

Fig. 4.



Here the dissidence between my results and those of Professor COHN, *who also experimented with hermetically sealed flasks, reappears.*

Numerous other experiments with pipette-bulbs and retort-flasks were made at the time here referred to, but it is unnecessary to record them. Suffice it to say that, like those just described, some of them corroborated and some of them contradicted the results of Dr. ROBERTS and Professor COHN.

§ 4. *Hay-infusions. Experiments with* COHN'S *Tubes.*

For reasons given by himself[*], Professor COHN deviated from the method of experiment pursued by Dr. ROBERTS, employing, instead of the pipette-bulbs, flasks, the nature of which will be understood from the following description. Let a zone of a common test-tube, about one third of its length from its open end, be softened by heat, and let the softened glass be drawn out so as to form a tube of much narrower bore than the original test-tube. Thus modified, the tube would consist of an elongated bulb below and an open funnel above, both being connected by a narrow neck (see fig. 5). Professor Cohn filled the elongated bulb to about two thirds of its volume with hay-infusion, plunged his bulbs in water, raised the water to ebullition, and continued the boiling for the required time. The tubes were then removed from their bath, and after being held open for a minute or two so as to allow the water condensed in their necks to evaporate, the funnel was plugged with cotton-wool.

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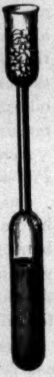
[*] Beiträge, July 1876, p. 256.

Fig. 5.



Professor Cohn considers that all possibility of external contamination is here shut out*. By his method, therefore, I wished to check the results above described. Accordingly, on the 24th of October, I had four groups of Cohn's tubes (twelve in a group) carefully charged with two freshly prepared hay-infusions. Each infusion was divided into two equal parts, one of which was neutralized and the other left in its natural acid condition. Twelve of the tubes were charged with one of the infusions neutralized, and twelve with the same infusion unneutralized. We will label this infusion A. Twelve other tubes were charged with the second infusion neutralized, and twelve with it unneutralized. We will call this infusion B. The forty-eight tubes were subsequently boiled for ten minutes in tin vessels containing water deep enough nearly to submerge them. Having proved by previous experiments that it was dangerous if not fatal to exactness to expose the infusions for one or two minutes to the air after their removal from the water, I took the precaution of plugging them first and removing them afterwards.

On the 28th of October (that is to say, four days after their preparation) several of the tubes containing the unneutralized infusion A were faintly but distinctly turbid and thinly covered with scum. The twelve neutralized tubes of the same infusion were at the same time perfectly clear. This retarding influence of the alkali has been of frequent occurrence in this inquiry. That it was simply a case of retardation was proved by the fact that, on the 30th of October, the twenty-four tubes, both neutral and acid, of infusion A were turbid and covered with scum.

On the same date the twelve neutralized tubes of infusion B were perfectly clear and without a trace of scum. Of the twelve unneutralized tubes three had given way, and a fourth yielded on the 31st. Four days later three of the neutralized tubes also yielded. The permanent state of matters was that eight out of the twenty-four tubes charged with infusion B had become turbid, while sixteen of them remained perfectly clear. I do not doubt that the tardy infection of some of the tubes just referred to arose from external contamination, which is almost inseparable from the method of experiment.

Here, while infusion A *corroborated Professor* Cohn, *infusion* B *in substance contradicted him.*

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* "Ehe ich über die Organismen berichte, welche sich in den gekochten Aufgüssen entwickelten, will ich bemerken, dass an eine nachträgliche Infection derselben durch von aussen nach dem Kochen eingeschleppte Keime bei unseren Versuchen nicht zu denken ist" (p. 259). I may remark that, with an atmosphere like that in which my recent experiments were conducted, there would be no chance of escape for an infusion thus handled.

§ 5. *Hay-infusions (in Closed Chambers).*

In dealing with hay-infusions I also fell back on the method of experiment which was found so effectual last year*, employing closed chambers in which the air had been permitted to cleanse itself by the gradual subsidence of its floating matter.

On the 3rd of October, 1876, my experiments with such chambers recommenced. Two of them, containing each three large test-tubes, were then charged with an infusion of hay accurately prepared according to the prescription of Dr. Roberts. Its specific gravity was 1006; it was superneutralized to the proper extent with caustic potash, but the period of boiling, instead of being three hours, was five minutes.

Examined from time to time for more than four months subsequently, the infusion in both chambers continued perfectly unchanged. It was free from suspended matter, free also from every trace of scum, maintaining for the light which passed through it a singular transparency.

Here, to a certainty, a period of boiling not amounting to one twentieth of that required by Dr. Roberts sufficed to destroy totally the power of generating life in an alkalized hay-infusion.

This result is in perfect harmony with *all* the results of last year. Chamber after chamber was then charged with infusions of hay, which were afterwards subjected to the boiling temperature for five minutes. In every chamber the infusion remained perfectly clear until purposely infected from without. There was no instance observed last year in which five minutes' boiling failed to sterilize hay-infusion, whether neutralized or unneutralized.

Thus, on the 26th of November, 1875, a group of three test-tubes was charged with hay-infusion of the same specific gravity and of the same degree of alkalinity as that found most resistant by Dr. Roberts. They were protected by glass shades, the air within the shade being calcined by an incandescent platinum wire in the manner described in my last paper†. The tubes were boiled for five minutes, the subsequent intrusion of contaminated air being prevented by a ring of cotton-wool. Thirteen months afterwards the infusion, greatly concentrated by evaporation, exhibited its pristine deep transparency. A second similar group of tubes was charged with alkalized hay-infusion on the 27th of last January, and on the 5th of December (that is to say, after a period of more than ten months) the infusion was found perfectly clear.

A number of hermetically sealed tubes charged with the same infusion, and boiled for only three minutes, have maintained for more than a year both their primitive transparency and their water-hammer sound. Thus many of the earliest experiments of the present year and the whole body of last year's experiments are in complete harmony with each other.

This harmony was, however, disturbed by some of the foregoing experiments with bulbs and tubes, and it was soon to be further disturbed by experiments with closed chambers. On the 6th of October, 1876, for example, an infusion was got ready in strict imitation of that prepared on the 3rd; it was of the same specific gravity, it was alkaline to the same degree, and it was introduced in the same manner into a chamber of three tubes; but whereas the infusion of the 3rd remained intact for months, and would have remained so indefinitely, a week had not elapsed before every tube of this new infusion was turbid and covered with fatty scum.

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* Briefly described in the Introduction.

† Phil. Trans. vol. clxvi. p. 50, and § 12 of this memoir.

§ 6. *Desiccation of Germs. New hay and old.*

In his work entitled 'Evolution, and the Origin of Life,' Dr. BASTIAN affirms, with repeated emphasis, that living matter is unable to maintain its life when exposed to a temperature even below that of boiling water. He refers to the scalding of the hand and other destructive effects, and also to the action of boiling water on eggs. He also refers to the experiments of SPALLANZANI on seeds, and extends the results observed with living matter of these special kinds to living matter generally. "It has been shown," he writes*, "and is believed by the great majority of biologists, that the briefest exposure to the influence of boiling water (212° F.) is destructive of all living matter." But scientific literature is not without examples which invalidate the inference drawn by Dr. BASTIAN from his special illustrations.

More than ten years ago an extremely significant observation directly bearing upon this subject was made by the wool-staplers of Elbœuf in France. They were accustomed to receive dirty fleeces from Brazil, and among other matters entangled in the wool were the seeds of a certain plant called *Medicago*. It had been repeatedly found by the wool-cleaners that these seeds sometimes germinated after a period of four hours' boiling. The late M. POUCHET repeated the experiment. He collected the seeds, boiled them for four hours, and sowed them afterwards in proper earth. To his astonishment they proved fruitful. He then closely examined the boiled seeds, and found the great majority of them swollen and disorganized; but amongst these ruined seeds he observed others which had refused to imbibe the water or to swell or break up in any way. These he carefully picked out, and sowed them and their neighbours separately in the same kind of earth. The swollen seeds were incapable of germination, while the unaltered ones rapidly gave birth to a crop. This was the only instance of such resistance known to POUCHET when he communicated the fact to the Paris Academy of Sciences.

The observation here described stands recorded in the 'Comptes Rendus' for 1866, vol. lxiii. p. 939, and it subverts the arguments founded by Dr. BASTIAN on the particular cases which he has adduced. It is not difficult, indeed, to see that the surface of a seed or germ may be so affected by desiccation and other causes as practically to prevent contact between it and a surrounding liquid †. The body of a germ, moreover, may be so indurated by time and dryness as to resist powerfully the insinuation of water between its constituent molecules. It would be difficult to cause such a germ to imbibe the moisture necessary to produce the swelling and softening which precede its destruction in a liquid of high temperature.

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* 'Evolution,' p. 46.

† In this connexion a remark of Dr. ROBERTS regarding the resistance of chopped green vegetables merits quotation. "The singular resistance of green vegetables to sterilization appears to be due to some peculiarity of the surface, perhaps their smooth glistening epidermis, which prevented complete wetting of their surfaces."

In my last paper I made some remarks upon this subject[*]; and in relation to our present experiments, the influence of drying and hardening was brought home to me by the fact that in all the foregoing cases the infusions which five minutes' boiling proved sufficient to sterilize *were, without exception, derived from fresh hay mown in* 1876, *while the infusions which five minutes' boiling failed to sterilize were derived, without exception, from old hay mown either in* 1875 *or some previous year.*

In the earlier experiments of the present inquiry this distinction between old and new hay came most clearly and definitely out. The result was subsequently blurred by circumstances which it required time and labour to unravel, and which will require patience on the reader's part if he would follow them through all their monotonous obstructiveness. They will, however, throw far more light upon the real character of these inquiries, and do more to reconcile the discords to which they have given birth, than if every experiment had been a success unshaded by doubt.

§ 7. *Hay-infusions. Further experiments with Closed Chambers.*

With a view to probing to the uttermost this question of drying and hardening, on the 6th of October an extensive series of experiments with closed chambers was begun. Three different kinds of hay were employed:—1st, Old hay, from Heathfield, Sussex[†]; 2nd, new hay from Heathfield (both, it may be stated, from a somewhat ungenerous soil); 3rd, new hay purchased in London, and artificially dried for some days upon a sand-bath. For these experiments eleven closed chambers were prepared, as I wished every result to be based as far as possible upon the testimony of two chambers. On the 6th of last October they were carefully charged with the infusions, the period of boiling afterwards being five minutes.

Two chambers were devoted to the acid and two to the alkalized infusion of old hay. Two chambers were also devoted to the acid and two to the alkalized infusion of dried hay. Two chambers were finally devoted to the alkalized and one to the natural acid infusion of new Heathfield hay.

Examined from day to day, differences were soon observed, not only between the different infusions, but also between different chambers containing the same infusion. Thus every tube of both the chambers containing the neutralized infusion of old hay became turbid, but the three tubes of the one chamber were loaded in four days with a fatty scum, while the tubes of the other chamber remained for ten days perfectly free from scum. The two chambers containing the acid infusion of old hay exhibited similar differences. Every tube in both of them became turbid; but in one of them the infusion was scumless throughout, while in the other each of the three tubes was heavily laden with scum.

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[*] Phil. Trans. vol. clxvi. p. 60.

[†] After the possible influence of hard drying and hardening had suggested itself, I purposely introduced old hay from various localities into the laboratory.

The two chambers containing the alkalized infusion of dried London hay had all their tubes turbid and covered with scum. In the case of the acid infusion of dried hay, the tubes of one of the chambers became turbid, while the tubes of the other chamber remained clear.

The two chambers of alkalized new Heathfield hay-infusion were also in disaccord. In the one chamber all three tubes became turbid and covered with scum, while in the other chamber the three tubes remained sensibly clear and free from scum. Nor did the three tubes of the single chamber charged with the new Heathfield acid infusion present the same appearance; for while one tube became thickly turbid, the other two remained perfectly pellucid.

Amid this confusion, the only point worth dwelling on is, that while no single case of escape occurred with the old-hay infusion, whether acid or neutral, with the infusions of both dried and undried new hay a certain percentage of the tubes remained sterile.

Reflection on these results naturally drew suspicion upon the chambers. They had been used before, and, though carefully cleansed, some unobserved source of infection may have clung to them. This, at all events, seemed the most rational way of accounting for the differences observed between samples of the self-same infusion placed in different chambers. Hence my desire to expose a fresh series of infusions in chambers which had never been used before.

Six new ones were therefore constructed, each of them containing six tubes. These were charged on the 3rd of November with infusions of old London hay, old Heathfield hay, new London hay, and dried London hay. Two chambers were devoted to each infusion, which in the one chamber was neutralized and in the other unneutralized.

The six tubes in each chamber were arranged in two rows of three tubes each. Those nearest to the glass front were called the front tubes, the others the back tubes. The infusion intended for the unneutralized chamber was unboiled before its introduction into the three back tubes, and boiled in those tubes for five minutes afterwards; the infusion for the front tubes was boiled for fifteen minutes before introduction and for five minutes afterwards. These differences in the mode and period of boiling were adopted to ascertain whether they had any influence on the subsequent development of life. In the case of the neutralized chambers, the infusion for the three back tubes was boiled for fifteen minutes outside before neutralization, and five minutes in the chamber after neutralization. The infusion for the three front tubes was boiled fifteen minutes outside after neutralization, and five minutes afterwards in the chamber. If the potash used for neutralization carried germs into the infusion, the difference between five and twenty minutes in the period of boiling might, it was thought, declare itself in the subsequent phenomena.

Four days after its introduction the old Heathfield acid infusion was found turbid throughout and covered with scum. The scum and turbidity were sensibly the same in all the tubes, though the period of boiling varied from five to twenty minutes. On the same day the neutralized infusion of the same hay was perfectly brilliant and free from scum. Three days subsequently, however (that is to say, on the 10th of November), the neutralized tubes also became turbid and covered with scum.

The salient fact here to be noted is, that in neither the neutral nor the acid chamber did a single tube of the old Heathfield hay-infusion maintain its primitive clearness and freedom from scum.

The old London hay behaved substantially as the old Heathfield hay, no single tube escaping either in the neutralized or the unneutralized chamber.

The dried new London hay comes next. A week after its introduction every one of the six tubes containing the acid infusion was turbid and coated with scum. In the neutralized chamber, on the contrary, two only of the back tubes gave way, the third back tube and the three front tubes remaining clear.

On the 3rd of November, moreover, a new chamber of six tubes was charged with an infusion of new London hay. Three of the tubes were neutralized and three unneutralized. Both infusions were introduced into the chamber unboiled, and were boiled afterwards for five minutes. In a week all the tubes had given way, becoming turbid in the same degree and covered to the same extent with scum. The newness of the hay had failed to secure the sterility of the infusions.

Nothing of this kind occurred in the experiments of last year. It was then found that hay-infusions of all kinds were uniformly sterilized by five minutes' boiling.

Guided by such hints as the experiments furnished, I continued to work. On the 4th of November four closed chambers of three tubes each were charged with infusions of old and new Heathfield hay—two chambers with the one, and two chambers with the other. One chamber of each pair contained a neutralized, the other an unneutralized infusion, and the time of boiling was ten minutes. Six days subsequently the infusion of new hay, both neutralized and unneutralized, was found perfectly unchanged. Of the old-hay infusion, on the other hand, only one of the six tubes escaped. The three acid tubes became completely turbid, while two out of the three neutral ones fell into the same condition.

§ 8. *Experiments with soaked Hay.*

Pondering still further on the influence of drying and hardening, and recognizing the necessity of not only wetting but also softening the germs, the thought occurred to me of soaking the hay for some days prior to digesting it. Old London hay was accordingly chopped up and placed in three glass vessels—one containing distilled water, another acidulated water, and a third alkalized water. The superior extractive power of the alkalized liquid was at once manifest; it rapidly assumed a dark colour. The distilled water came next, yielding a colour less deep than that of the alkalized, but more deep than that of the acidulated water. The alkaline and distilled-water infusions emitted a rich odour of hay, while the smell of the acid infusion was very faint, and not like that of hay. The hay was permitted to soak from the 8th to the 11th of November. It was then digested for three hours in the same liquid at a temperature of 120° F., boiled, filtered, and introduced into the closed chambers, where it was reboiled in each case for five minutes.

Prior to digesting the hay in the liquid in which it had been soaked *Bacteria* had developed in swarms. These, of course, were killed by the boiling, and they were not entirely removed by the filtration. The alkaline infusion, indeed, though filtered repeatedly, was sufficiently turbid to prevent the flame of a candle placed behind the tubes containing it from being seen. The same to a less extent was true of the distilled-water infusion. This latter had been divided into two portions, one of which was accurately neutralized, and the other left unneutralized, separate chambers being devoted to each.

From the 11th to the 18th of November the only change observed in any of the infusions was in the direction of increased transparency. They all became clearer with time, the distilled-water infusions becoming particularly clear and brilliant at the top. After two or three days' quiet the alkaline infusion allowed a flame placed behind it to be seen of a deep and brilliant red. The acidulated-water infusion remained entirely unchanged; but this is not worth dwelling on, for in this case, even when exposed to the common air, the infusion resisted infection for a considerable time.

In no case was the fatty scum which had been already so frequently observed formed in any one of the tubes. Some change inimical to the particular organisms which produce this scum must have been caused by the soaking of the hay.

Examined microscopically on the 18th of November these infusions, I thought, exhibited undoubted evidences of Bacterial life. Bacterial forms were unquestionably there in considerable numbers, more particularly in the sediment at the bottoms of the tubes. Nor do I now see any valid grounds for doubting the presence of life; but I was warned against drawing too hastily the conclusion which first prompted itself, by boiling an infusion swarming with active *Bacteria*, and submitting the liquid after cooling to microscopic examination. Here also the dead Bacterial forms were preserved, and it was extremely difficult to distinguish their motions, which were certainly Brownian motions, from those observed in the protected infusions of soaked hay.

The experiment was thought worth repeating. On the 16th of November accordingly chopped bundles of old Heathfield hay and new Heathfield hay, and of old London hay and new London hay, were placed in glass dishes containing distilled water, and were thus soaked until the 18th. They were then moved from the lower laboratory, and taken, with their glass covers, to a distant room at the top of the Royal Institution. Here the four specimens of hay were digested for three hours at a temperature of 120° Fahr. They were filtered, boiled, refiltered, some of them through 100 layers of filter-paper; after which they were introduced into four closed chambers of six tubes each, and then boiled for five minutes.

On the 20th of November the infusions in all the chambers appeared to be as free from organisms as at first. The new Heathfield and the new London hay-infusions in their respective chambers had their somewhat turbid columns surmounted by an exceedingly clear zone of liquid, due, I should consider, to the mechanical subsidence of the particles, had not subsequent experience taught me to regard this appearance as a sign of life.

On the 23rd scum had begun to gather on every tube of the case containing the infusion of old Heathfield hay. On the 30th this scum continued, but there was no trace of it in any of the chambers containing new Heathfield hay, new London hay, and old London hay. These infusions were all somewhat turbid; but the turbidity differed very little from that exhibited when the infusions were prepared.

I spent a good deal of time over these infusions of soaked hay, both with the microscope and otherwise, but the recorded observations would not add materially to our knowledge. I therefore dismiss them with the remark that their general drift was in favour of the idea that the extraordinary resistance to sterilization manifested by the old-hay infusions is the result of hardening and desiccation. The foregoing observations, however, have been noted, more with the view of indicating my line of thought than of claiming for them any value whatever as a demonstration.

§ 9. *Infusions of Fungi.*

Turning from hay to substances in which germs, if they existed, could not be desiccated, I felt pretty sure that infusions of such substances would be unable to resist the boiling temperature. To test the correctness of this view the following experiments were made:—Three different kinds of fungi (red, black, and yellow) were gathered in Heathfield Park on the 13th of October, and digested separately in London on the following day. Three tubes of a closed chamber containing six tubes were charged with the red-fungus-infusion and three with the black, while a second chamber of three tubes was charged with the yellow-fungus infusion. They were all boiled for five minutes after their introduction into the chambers.

For two or three days all the infusions continued clear; but, contrary to my expectation, they subsequently broke down, every tube of the nine becoming turbid with organisms and covered with scum.

Examined microscopically on the 8th of November the red-fungus infusion was found charged with a multitude of spore-like bodies, massed in some places continuously together, in others floating freely in the liquid. Among these ran long filaments, dotted with spore-like specks from beginning to end. There was a considerable number of Vibrios in one of the tubes. The black-fungus infusion contained a mixed population of Vibrios and *Bacteria* with spore-filled filaments. Swarms of *Bacteria* were observed in the red-fungus infusion.

Suspicious of the chambers in which these infusions had been exposed, I had three new ones constructed and provided with new tubes. A fresh supply of fungi was sent to me from Heathfield, a tree fungus being, however, substituted for the black one used in the former experiments. On the 1st of November the three infusions were very carefully introduced into three chambers, a chamber being devoted to each infusion. I thought it advisable to vary the period of subsequent boiling. One tube of the yellow fungus was therefore boiled for five, one for ten, and one for fifteen minutes; but as it was difficult to save the infusion from waste when the boiling was long continued, one tube of each of the other two infusions was boiled for five minutes, and the other two for ten. Tubes charged with the respective infusions were exposed at the same time to the common air.

In two days the outside tubes containing the red- and yellow- fungus infusion became turbid and covered with the fatty scum so prevalent in our laboratory this year. No scum had formed on the surface of the exposed tree-fungus infusion, which, to casual observation, appeared quite black. Closer scrutiny, however, showed that it transmitted the deepest red of the spectrum, and was apparently quite free from floating matter. It changed rapidly during the night of the 3rd, and on the morning of the 4th of November the bottom of this tube was found laden with a heavy dark-brown precipitate, while numerous dark-brown flocculi floated in the liquid overhead, which had become almost as clear and colourless as water. Under the microscope the dark-brown mass resolved itself into confused moss-like patches and long cylindrical sheaths dotted throughout with small dark specks. These filaments with spore-like specks have been of very frequent occurrence in this inquiry.

The deportment of the closed chambers was as follows:—1. Yellow fungus: the liquid in the three tubes remained perfectly and permanently clear and without a trace of the scum which loaded the infusion outside. 2. Red fungus: one of the three tubes became thickly turbid, while the two others maintained their pristine brilliancy. 3. Tree fungus: one of the tubes became thickly turbid, the two others remained permanently clear.

I asked myself why should one tube of the red fungus give way and the others remain intact? The answer seemed at hand. The turbid tube had been boiled for only five minutes, while the clear ones had been boiled for ten. On consulting the adjacent chamber this possible explanation was blown to the winds, for here the turbid tube had been boiled for ten minntes, while its untainted neighbour had been boiled for only five.

Thus, although the more careful repetition of the experiments did not secure every tube from infection, the escape of seven out of nine of them entirely destroys the presumption of spontaneous life development which the first experiments might seem to suggest.

Wishing to observe more attentively the action of common uncleansed air upon boiled fungus-infusions, a tray of 100 tubes was charged with them on the 14th of October. Thirty-five tubes were filled with black, thirty-five with yellow, and thirty with red-fungus infusion. On the 16th of October every one of the yellow-fungus tubes was turbid and covered with a thick, coherent, cobweb-like scum. The surfaces of the black-fungus tubes were also sprinkled with spots of white scum. Turbidity was the only change observed in the red-fungus tubes. They were wholly free from scum.

Examined microscopically on the 2nd of November the yellow-fungus tubes were for the most part found swarming with exceedingly small and active *Bacteria*; the red-fungus tubes also swarmed with *Bacteria*, some beaded Vibrios being mingled with them. In many of the tubes examined galloping monads appeared, attaining an astounding development in the black-fungus infusion. Patches of moss-like matter would appear here and there in the field of the microscope; and it was no uncommon thing to see from ten to twenty monads nestling and quivering in this "moss," and darting actively in and out of it. They put me in mind of frogs amid their spawn; and as I looked at them my belief in the animality of the one was almost as strong as in that of the other. Almost every patch of spawn-like matter had its colony. In some cases hardly any thing but monads was to be seen; but in others the crowding of active Vibrios was so great that the monads wholly retreated from the field.

§ 10. *Infusions of Cucumber, Beetroot, &c.*

The fungi having disappeared on the approach of winter, I turned to cucumber and beetroot, not expecting that their sterilization would offer any difficulty. Two closed chambers were accordingly prepared, left for the proper time in quietness, and on the 7th of November were charged, the one with the cucumber- and the other with the beetroot-infusion. In a few days the infusions in both chambers broke down, first losing their transparency and afterwards loading themselves with fatty scum.

On the 18th of November twenty-four COHN'S tubes* were charged with infusions of cucumber, beetroot, parsnep, and turnip, six tubes being devoted to each infusion. They were placed in a vessel of cold water, raised gradually to the boiling-point, and maintained at the boiling temperature for ten minutes. Before their removal from the hot liquid they were one and all plugged with cotton-wool.

On the 30th of November all the infusions were thickly turbid throughout and heavily coated with scum.

From some of the precautions already mentioned it may be inferred that before this point of the inquiry had been reached, I had begun to suspect the atmosphere in which I worked. Hay of various kinds, both old and new, had been exposed and shaken about in the laboratory, the air of which doubtless contained multitudes of spores which diffused and insinuated themselves everywhere. So, at all events, I reasoned. On the 20th of November, therefore, I had infusions of cucumber, beetroot, parsnep, and turnip prepared, far from the laboratory, in one of the highest rooms of the Royal Institution, and introduced into four new chambers of three tubes each. I deemed the precaution of preparing the infusions and introducing them in the distant room sufficient. Accordingly, when the chambers were charged they were carried down, and the infusions boiled in the laboratory.

Two days afterwards the parsnep alone remained clear. This, however, was only a respite, for a day or two subsequently it fell into the condition of its neighbours. On the 30th of November both turnip- and parsnep-infusions were turbid throughout, and laden at the surface with thick fatty scum. The cucumber was also heavily laden with scum, which sent long streamer-like filaments into the subjacent liquid. The beetroot agreed with the others in becoming turbid, but differed from them in remaining free from scum. In no case last year did turnip-infusion show the deportment here described. Knowing, then, from multiplied experiments, that turnip possessed no inherent power of life-development, I was forced to refer its present behaviour, and with it the behaviour of cucumber, beetroot, and parsnep, to infection from without.

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* See § 4.

I once more tried removal to a distant room, with the added precaution of not only introducing the infusions into the chambers upstairs, but of boiling them there. It had been noticed that when the test-tubes were withdrawn from the oil-bath, and the discharge of steam into the chambers ceased, a somewhat violent entrance of the air into the cooling-chamber was the consequence. To sift such air of its germs, both the funnel of the pipette and the open ends of the bent tubes were carefully stopped with cotton-wool. The wool was never removed from the funnel, and it was not removed from the bent tubes until the chamber had thoroughly cooled. The same vegetables were operated on, viz. cucumber, beetroot, turnip, and parsnep. On the 25th of November four chambers were charged with the infusions. On the 30th they were one and all covered with a layer of deeply pitted and corrugated fatty scum. Thus far, then, I was defeated in my efforts to escape contamination.

During these experiments a fact was observed which repeated itself afterwards in other instances. Samples of the different infusions were always exposed to the common air beside their respective chambers, and in general these outside samples became turbid and covered with scum a day or so before the interior tubes gave notice of breaking down; but here, in the case of the turnip, the outside tube continued pellucid and free from life for some time after the inside ones had become turbid with organisms. How could this be? The case of my two trays placed one above the other last year* suggested itself to my memory. In point of life-development it was then found that the lower tray was always in advance of the upper one. As pointed out at the time, the absence of agitation which permitted the germs to sink into its tubes was the cause of the quicker contamination of the lower tray. No other cause appeared to me assignable in the present instance. By some means or other germs had insinuated themselves into my closed chamber, where the tranquillity of the air permitted them to sink into the infusion, and thus produce effects in advance of those produced by the unquiet air outside. So I reasoned.

But how could the germs get into the chamber? I could fix at the moment only upon one way. The weather had changed from warm to cold and from cold to warm. This genial outside temperature sometimes caused the air surrounding the infusions to rise to upwards of 90° Fahr., and we had often to work in this heat. To moderate it, I sometimes partially turned off the gas, thus lowering the temperature of the room 10° or more. The contraction of the air within the closed chambers followed as a matter of course, and the bent tubes being open, I thought the entrance of the external air might be sufficiently rapid to carry germs along with it.

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* Phil. Trans. vol. clxvi. p. 68.

A new chamber of six tubes was therefore prepared upstairs, three of its tubes being charged with cucumber- and three with turnip-infusion on the 27th of November. The pipette funnel and the bent tubes were plugged above with cotton-wool, which was not removed from them afterwards. I took care, moreover, not to alter the gas-stoves in any way. My care was nugatory. In three days every tube of the six was laden with life. Another chamber of six tubes, charged on the 30th of November with cucumber-infusion, and two additional ones prepared on December 1st, shared the same fate.

Slices of cucumber were next digested for three hours; the infusion was filtered, boiled, and such precipitated matter as appeared on boiling was removed by refiltering. The liquid thus prepared was introduced into five thick glass tubes, which were hermetically sealed, placed in a cold oil-bath, gradually heated to 230°, and maintained at that temperature for a quarter of an hour. The tubes being removed and permitted to cool, the infusion was introduced into a chamber of six tubes, and boiled there for five minutes.

The superheating of the infusion did not even retard the development of life, for in less than two days every tube in the chamber swarmed with *Bacteria*. Thus far, then, every attempt at a solution was defeated.

But why, it may be asked, attempt such solutions? Was it mere prejudice against the doctrine of spontaneous generation that prevented me from frankly submitting to the apparent logic of facts, and admitting the experiments just recorded to be a demonstration of the doctrine? By no means. The only prejudice I feel is the wholesome repugnance to accepting momentous conclusions on insufficient grounds. HUME'S celebrated argument has its application here. Taking antecedent experience fully into account, it was easier for me to believe my knowledge imperfect, or my present work erroneous, than to believe the doctrine of spontaneous generation true.

§ 11. *New Experiments on Animal Infusions. Contradictory results.*

In the course of this inquiry I was continually reminded of last year's experiments, when the most complete immunity from Bacterial or fungoid life was so readily secured. I had operated many times with turnip, never finding the least difficulty as to its sterilization. It is certain that the care bestowed in preparing the turnip-infusion on the 20th of November, 1876, was greater than that bestowed upon the same infusion in 1875. But whereas the latter was invariably sterilized by five minutes' boiling, remaining afterwards as pellucid as distilled water, the former, three days after its preparation, became thickly turbid and swarming with life. I extended the present inquiry to other substances whose deportment was familiar to me last year, some of whose infusions, indeed, still remain with me as clear as they were on the day of their preparation.

On the 1st of December, for example, infusions of beef, mutton, pork, herring, haddock, and sole were prepared, and introduced into six closed chambers, each containing three tubes. On the 5th of December the pork, beef, mutton, and haddock were all covered with a fatty corrugated scum. A seventh chamber, containing artichoke-infusion, prepared at the same time, was found on the 5th more turbid than any of the animal infusions, and equally covered with scum. In the animal infusions, indeed, the body of the liquid underneath the scum maintained a surprising brilliancy, the development of life being confined to the layer in immediate contact with the atmospheric oxygen.

On the 5th of December the herring- and sole-infusions were both clear; but this was only a respite, for on the 6th white spots appeared on the latter, which extended until they covered the whole surface. The herring-infusion remained clear for a week, after which small specks began to appear on its surface. They never reached the development of the scum which coated the other infusions. It sometimes occurred to me that the oil of this fish exercises a certain antiseptic action.

Last year I preserved infusion of herring perfectly pellucid for months, even in a chamber so leaky that the light could be seen through its chinks. I had, moreover, no failure with any of the animal infusions here enumerated. Last year they all remained sweet and clear; this year, with far greater precaution, I failed to protect any of them from putrefaction. Reflection on these results, renders, I think, but one conclusion possible to the scientific mind. It will be loth to assume that mutton, beef, pork, haddock, herring, and sole had totally changed their natures, and contracted qualities and powers this year which they did not possess a year ago. But if the origination of the observed life be denied to the infusions themselves, there is but one other source to which it could be referred, namely, atmospheric contamination.

It became, indeed, more and more obvious to me that, in consequence of increased virulence in the contagia afloat this year, liberties in the preparation of the infusions or defects in the construction of closed chambers which would have been of no moment a year ago were sufficient to ruin the experiments, and render nugatory the usual means of sterilization. Against such defects I continued to struggle. With a view to stopping all chinks and crannies which might permit of the entrance of contamination, I had some of the chambers carefully coated with oil-silk and others covered with three coatings of strong paint; and as failure had attended my efforts to procure an uninfected atmosphere upstairs, I had the entire apparatus used for digesting, filtering, and boiling removed to a store-room at the base of the Royal Institution. The floor of the room was of stone, and it was covered by no carpet. Prior to going into it, moreover, I caused my assistant to remove the clothes which he had previously worn in the laboratory and to dress himself in others. The infusions prepared under these conditions were cucumber, melon, turnip, and artichoke, which, from beginning to end, were operated on below stairs. Two chambers were devoted to each infusion, and after the usual boiling in the chambers they were permitted to remain in the store-room throughout the night, being transferred to the warm laboratory next morning.

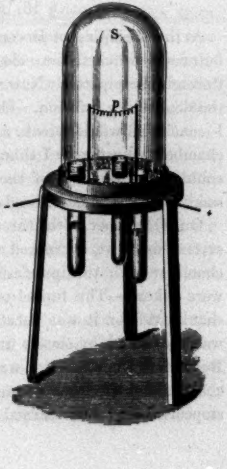
I fully expected that the majority of these chambers would prove sterile. I did not expect to find them all in this condition, because the chambers had been put together in the laboratory, the air of which must have deposited its germs not only on the glycerine-coated interior of the chambers, but also on the inner surfaces of the test-tubes. My expectation, moderate as it was, was not realized. The only noticeable peculiarity in the deportment of the infusions was that they yielded tardily, but in the end every one of them, without exception, broke down.

Was the infection in this case derived from the air of the store-room? I think not; and for this reason:—On the 27th of December four hermetically sealed flasks, charged with a cucumber-infusion which had remained perfectly pellucid for some weeks, were opened in the store-room; four similar flasks, charged with the same infusion, were opened at the same time in the laboratory. On the 31st of December the whole group of the latter four was found invaded by organisms, while those opened in the store-room contracted no infection and developed no life. I do not think, therefore, that the air of the store-room had any thing to do with the contamination of the infusions contained in the closed chambers, but that the contagium already existed in the chambers when they were taken down stairs. They acted as infected houses placed in a salubrious air.

§ 12. *Infusions protected by Glass Shades containing calcined Air.*

I have already described this mode of experiment[*]. The shades stood upon circular plates of wood, each supported on a tripod (see fig. 6). Under each shade were two upright rods of stout copper wire, and stretching from rod to rod was a spiral (*p*) of platinum wire. The copper wires passed through the slab of wood, their free ends being in the air. The rim of each shade was surrounded by a collar of tin attached by wax to the slab, with a space of about half an inch between the collar and the glass. After the introduction of the infusions and the mounting of the shades, this annular space was packed with cotton-wool. The aim here was to destroy the floating matter of the air by the incandescent platinum spiral. The air heated by the spiral would of course expand, passing outwards through the cotton-wool, while the air reentering, on the cooling of the shade, would be duly sifted by the wool. In my former experiments five minutes' incandescence sufficed to render the air absolutely inoperative on infusions exposed to its action.

Fig. 6.



In the present experiments the period of incandescence was doubled, ten minutes being allowed instead of five, while the wire was raised to the highest possible degree of incandescence. The infusions employed were turnip and cucumber, a group of three tubes being charged with each. After the air had been calcined, the infusions were boiled for five minutes in an oil-bath. With this mode of treatment not a single failure occurred last year, turnip-infusion being among the number of liquids thus treated. This year two days sufficed to render every one of the six tubes turbid with organisms and to cover the infusion with a heavy scum.

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[*] Phil. Trans. vol. clxvi. p. 50.

I, however, had occasion to doubt the closeness of these shades. The wax intended to seal the junction of the tin collar with the plate of wood had cracked and yielded here and there, and the entry of contamination through such cracks was possible. Six new shades were therefore mounted and surrounded by collars which were imbedded in white lead and firmly screwed down to the plate of wood. The height of the collar, which measured the depth of the filtering layer of wool, was much greater than it had ever been last year. As before, the period of incandescence was ten minutes, during which the platinum spiral was brought as close as possible to its point of fusion.

Each of these six shades covered a group of three test-tubes. Two such groups were charged with turnip, two with cucumber, and two with artichoke-infusion. The infusions, as usual, were boiled for five minutes after calcination. They were all brilliant when prepared; but in two days every one of them had become turbid, and had covered itself with a fatty scum. This gradually augmented until it reached in some of the tubes a thickness of half an inch. The weight of the scum caused it in some cases to bag downwards, forming a kind of inverted cone, the apex of which was more than an inch from its base. These bags finally broke and scattered their organisms in the subjacent liquid.

§ 13. *Further precautions against Infection.*

At the beginning of December, my attention being keenly aroused by those successive failures, I watched more closely than I had previously done the filling of the test-tubes through the pipette. Now and then I noticed minute bubbles of air carried down with the descending infusion. On escaping from the end of the pipette, these small bubbles I concluded would break, and scatter such germs as they contained in the air of the chamber. Last year I should have found it difficult to believe that a cause so small could lie at the root of the observed anomalies; but this year I had learned to respect small causes, and accordingly took measures to effectually exclude the air.

On December 4th three chambers, which had been previously left quiet for several days, were charged with carefully prepared cucumber-infusion, and two other chambers with turnip-infusion prepared with equal care. The following precautions were taken:—The funnel of the pipette formerly employed was broken off from its shank, and for it was substituted a "separation-funnel" with a glass stopcock. This was connected by closely fitting india-rubber tubing with the shank of the pipette. But before the connexion was made, the funnel was filled with the infusion, and the stopcock turned on for a moment, until the liquid issued from the orifice below. The stopcock being then turned off, the flow of the liquid ceased, and the column in the shank below the stopcock was supported by atmospheric pressure. A pinchcock nipped the india-rubber tube at its centre. The portion of tubing above the pinchcock being filled with the infusion, the end of the separation-funnel was introduced into the tube, all air being thus excluded. On turning on the stopcock and releasing the pinchcock, the liquid passed slowly down the shank of the pipette, filling it wholly. The point of the shank was then placed in succession over the test-tubes, the infusion entering them without a single associated bubble. The arrangement was not perfect, but it was an improvement upon previous ones. As before, the charged chambers were placed in a room the air of which was maintained at a temperature of about 90° Fahr.

The result was as follows:—Of the two turnip chambers prepared, as just described, on the 4th, one had completely given way on the 6th. In the other chamber two out of the three tubes had given way, but the third remained permanently brilliant. Previous to this series of experiments I had never succeeded in saving even a single tube of cucumber-infusion; here, however, two out of the three chambers charged with it remained perfectly clear for many days. Subsequently one of these chambers yielded in part, through an accident, but the other chamber is as brilliant at this moment as it was on the day of its preparation many months ago.

Now, as regards inherent power to generate life, the infusion of this chamber was in precisely the same condition as its two neighbours. They, one and all, contained the same infusion; and there is no way of accounting for the observed difference of deportment save by reference to contamination from without. Here we seem to be on the traces of the enemy which has given us so much trouble.

On the 5th of December two additional cases were charged with infusion of melon prepared in the usual way; and on the 12th of December I subjected both these chambers, and those prepared upon the 4th, to a very close scrutiny. The result was instructive. After the introduction of the infusions, and prior to the removal of the separation-funnel, the india-rubber tubing connecting the latter with the shank of the pipette was perfectly closed by the pinchcock. Provided the clasping of the india-rubber tube round the shank of the pipette were perfectly air-tight, the liquid contained in the shank ought to remain there, supported by atmospheric pressure. If, however, the india-rubber tube failed to clasp with sufficient tightness the pipette-shank, air would insinuate itself between the two, and the depression of the liquid would be the consequence. The result observed upon the 12th was this:—In two only of the seven chambers prepared on the 4th and 5th was the liquid column found perfectly supported; and only in these two chambers were the test-tubes, which contained cucumber-infusion, without exception pellucid.

In the five remaining chambers the liquid columns, which had completely filled the pipette-shanks on the 4th and 5th, were found more or less depressed. The tubes in one of the chambers, containing melon-infusion, had become rapidly turbid and covered with scum. The pipette-shank in this case was found entirely emptied of its liquid and filled with air. Another chamber had nine inches, while a third had seven inches of its pipette-shank filled with air. In a fourth chamber only one inch of the pipette-shank was filled with air; here one out of a total of three tubes remained pellucid. Thus, where the closure above was perfect, we had in this instance perfectly pellucid infusions; where it was grossly defective, the infusions gave way in all the tubes; while where the closure was but slightly defective we had the escape of a fraction.

The defects thus revealed came doubtless into play when the infusions were introduced, the descending column of liquid sucking in minute air-bubbles between the india-rubber tubing and the pipette, thus carrying with it the external contagium. Few are aware of the precautions essential to save the experimenter from error in inquiries of this nature. Even with some of our best and most celebrated observers I find no adequate sense of the danger involved in their modes of experimentation.

§ 14. *Experiments in the Royal Gardens, Kew.*

But it was only in exceptional instances, dependent on the state of the air, that even precautions such as those described in the foregoing section secured freedom from contamination. The contagium seemed omnipresent and persistent, and whether it was local or general—due to the accidental condition of our laboratory, or to an epidemic of the air—became a question with me, not by any means to be decided offhand. On this point, then, I held judgment in suspense. The infection was, to all appearance, fully accounted for by reference to the conditions under which I worked; but as regards outbreaks of epidemics the autumn had been a remarkable one, and it seemed well worth investigating whether it was not also a period prolific generally in the germs of putrefaction.

I resolved therefore to break away wholly from the Royal Institution, and, thanks to the friendly permission of the President of the Royal Society, I was enabled to transfer my apparatus to Kew Gardens. By the enlightened munificence of Mr. JODRELL, a new and very complete laboratory had been just erected there, and in it I sought a purer air than I could find at home.

My chambers hitherto had been constructed of wood, but those to be tested at Kew were made of block-tin, and they were carried direct from the tinman's to the gardens without being permitted to come near the infected air of Albemarle Street. At Kew the test-tubes employed were first cleansed with carbolic acid, then washed with a solution of caustic potash, afterwards swept out with distilled water, and finally raised almost to the temperature of redness by a Bunsen-flame. They were then fitted air-tight into the chambers with white-lead and tow.

The chambers were closed on the 3rd of January, and allowed to remain quiet until the 8th, when the two most refractory liquids that I had encountered in the laboratory of the Royal Institution were introduced into them. These were infusions of cucumber and melon. There were two chambers devoted to each infusion—four in all; and each chamber embraced three large test-tubes. The period of boiling was that found effectual last year, *i. e.* five minutes. The temperature of the room in which the chambers were placed was maintained, partly by hot-water pipes and partly by a gas-stove, at about 90° Fahr.—a temperature which had been proved eminently favourable to the development of *Bacteria*.

Tubes containing the same infusions were at the same time exposed to the common air of the Jodrell laboratory. These became rapidly turbid and covered with scum. Anxiously inspected during the early days of their trial, the protected tubes showed no signs of giving way. Nor did they yield afterwards. On the 19th of January the four chambers were removed in a van from Kew, and shown in the evening of that day to the members of the Royal Institution, including many Fellows of the Royal Society. The infusions were one and all brilliant, no trace either of turbidity or scum being found associated with any of them. During all my previous efforts (and they had been very numerous) I had never succeeded in saving a single tube of melon-infusion; here, however, every tube of both chambers was intact. The epidemic was thus localized, the obvious cause of it being the contaminated air of our laboratory.

A couple of days subsequent to the removal of the chambers from Kew, a single tube of the cucumber-infusion became turbid, its two neighbours in the same chamber remaining intact. As long as they were kept quiet not one of the other tubes, either of melon or cucumber, gave way. They all remained as pellucid as at first. Their removal from Albemarle Street to the city last year ruined many of our sterilized chambers. I was not therefore prepared to see so little damage done by the transport from Kew.

It may be remarked, in passing, that this infection of an infusion by mere mechanical shaking is an obvious proof that the contagium is not a gas or vapour, but that it consists of particles capable of being detached from the interior surface of the chamber, and endowed with the power of passing into active life.

Two other chambers were exposed at the same time in the Jodrell laboratory, the one containing beef- and the other sole-infusion. They are by no means so sensitive as the cucumber and melon, still one of the three beef-tubes broke down, becoming thickly turbid throughout. Right and left of this tube its two companions remained perfectly transparent. As an illustration of the externality of the contagium, the result was more conclusive than it would have been had all three tubes remained intact; for had the power of developing the organisms which produced the turbidity been inherent in the infusions, its action would not have been confined to a single tube.

It will be understood that when the chamber is lifted from the oil-bath in which its infusions are boiled, the air within the chamber contracts, and an indraught is the consequence. If the entering air be properly sifted, by passing it through cotton-wool plugs, no harm is done; but if it enter an aperture unsifted, it carries its motes along with it. In the beef-chamber just referred to an aperture of this kind, about the size of a pin-hole, was detected. This obviously was the door through which the contagium entered. Through a similar but graver defect in its chamber the sole-infusion also broke down; but in a subsequent experiment with sole-infusion in the Jodrell laboratory, two thirds of the whole number of tubes charged with it remained free from all trace of life.

§ 15. *Experiments on the Roof of the Royal Institution.*

With a view to making nearer home experiments similar to those made at Kew, I had a wooden shed erected on the roof of our laboratory. The shed was provided with benches, water and gas-pipes, and a stove for heating. To an infusion of cucumber, which I had found extremely intractable in the laboratory, my attention was first directed. Two tin chambers of three tubes each were prepared, and transferred to the shed from the workshop where they were made without being permitted to enter our laboratory. The cucumber used for the infusion was also kept clear of the infected air; it was sliced and digested in the shed, the infusion was there filtered, introduced into the tin chambers, and boiled subsequently for five minutes.

The result was not that expected. Not a single tube of either of these two chambers escaped contamination. They one and all behaved like the same infusion in the infected laboratory, becoming in three days turbid throughout and laden with fatty scum.

I have been daily and hourly impressed with the parallelism between these phenomena of putrefaction and those of infectious disease. A further illustration of this parallelism is here presented to us. The clothes of my assistants who prepared the infusion in the shed had been worn in the laboratory, a transfer of infection by one of the modes of transfer known to every physician being the result. The thoughtful physician cannot indeed fail to see the absolute identity of deportment between the contagia with which he is familiar, and those assailants of my infusions against which I have been contending so long.

With regard to the shed my first step, after this preliminary failure, was to disinfect it. This was done by washing every part of it, first with a mixture of carbolic acid and water, and secondly with a solution of caustic potash. When the whole was well dried, new tin chambers furnished with new tubes were introduced. Cucumbers and beef fresh from the market were also digested in the shed, my assistant taking care to cover his legs with clean linen trowsers, and his body with a new blouse. There was one chamber devoted to the cucumber and another to the beef. Into the former the infusion was introduced on the 19th, and into the latter on the 20th of March; each infusion was boiled for five minutes after its introduction.

Let us compare results and draw conclusions. At a distance of eight yards from the shed, viz. in the laboratory, infusions both of beef and cucumber refuse to be sterilized by three hours' boiling. Indeed I have samples of both infusions which have borne five hours' boiling and developed multitudinous life afterwards. But the upshot of this experiment in the disinfected shed is, that every tube of the two chambers, though boiled for only five minutes, contains an infusion which, at the present hour, is as limpid as the purest distilled water.

What shall we say, then? is the infusion in the laboratory endowed with a generative force denied to the same infusion in the shed? Irrespective of the condition of the air, can a linear space of eight yards produce so remarkable a difference? It is only the confusion of mind still prevalent in relation to this subject that renders such a question necessary. Let me add that it suffices simply to wave a bunch of hay in the air of the shed to make it as infective as the laboratory air. Even the unprotected head of my assistant when his body was carefully covered sufficed in some cases to carry the infection.

If any thing were needed to illustrate the extraordinary care necessary on the part of physicians and surgeons, both as regards the clothes they wear and the instruments they use, such illustrations are copiously furnished by the facts brought to light in this inquiry.

§ 16. *Preliminary Experiments on the Resistance-limit of Germs to the temperature of Boiling Water.*

While continuing the conflict and experiencing the defeats recorded in the foregoing pages, a remark of Professor LISTER's sometimes occurred to me. To apply the antiseptic treatment with success, the surgeon must, he holds, be interpenetrated with the conviction that the germ-theory of putrefaction is true. He must not permit occasional failures to produce scepticism, but, on the contrary, must probe his failures, in the belief that his manipulation, and not the germ-theory, is at fault. This may look like operating under a prejudice; but Professor LISTER's maxim is nevertheless consistent with sound philosophy and good sense; and if I permitted a bias to influence me in this inquiry, it was one fairly founded on antecedent knowledge, which led me to conclude that the long line of failures above referred to would eventually be traced to my ignorance of the conditions whereby perfect freedom from contamination was to be secured.

I laboured to discover these conditions, and to learn something more regarding the nature of the contamination—its origin, persistence, and manner of action. When these researches began, five minutes' boiling, as I have frequently stated, sufficed to sterilize the most diversified infusions. Here we have frequently extended the time of boiling to ten and fifteen minutes, and, in some cases glanced at above, to immensely longer periods, without producing this result. I desired more exact knowledge as to the limit of endurance, and with this view, on the 22nd of December, had six bulbs charged with an infusion of cucumber, sp. gr. 1004. They were then plugged with cotton-wool, hermetically sealed, and subjected to the boiling temperature for 10 minutes. Six other bulbs, charged with the same infusion and treated in the same way, were boiled for 30 minutes. Finally eight bulbs, similarly charged, were boiled for 120 minutes.

On the 23rd of December three of the first group of bulbs, three of the second, and five of the third, having their sealed ends filed off, were exposed to a tolerably constant temperature of about 90° Fahr. Not one of these twenty bulbs preserved itself free from life. On the 25th of December every one of them had given way to cloudiness and turbidity.

There was, however, a marked difference between the sealed and the unsealed bulbs.

To the latter, it will be remembered, the air had access through the plug of cotton-wool, while to the former no air had access, save the small quantity imprisoned above the infusion when the necks of the bulbs were sealed. The aerated bulbs grew rapidly and thickly turbid, while a passing cloudiness was all that showed itself in the sealed ones. This soon disappeared, and left the infusions apparently intact. In fact it required some attention to detect the appearance of this fugitive life, which existed only so long as there was oxygen to sustain it. I have ranged the sealed and unsealed tubes side by side in groups. To the most cursory observation the difference between them is obvious. The experiment strikingly illustrates the dependence of the special organisms here implicated on the oxygen of the air.

The experiments were pushed still further on the 28th of December. Two bulbs of cucumber, two of melon, two of turnip, and two of artichoke were then plugged, sealed, and maintained at the boiling temperature for four hours. Six of the eight bulbs burst in the operation, but two of them, a bulb of melon and one of cucumber, bore the ordeal uninjured. After cooling, their sealed ends being broken off, they were placed in the warm room. The melon remained permanently sterile, but in two days the cucumber-infusion became turbid and laden with fatty scum.

Eight similar bulbs were boiled on the same day for five hours and a half. Four of them burst, but four remained intact. Of these, two contained cucumber-, one melon-, and one turnip-infusion. Three out of the four bulbs were sterilized by the long-continued boiling, but one cucumber-bulb passed through the ordeal unscathed. Two days after the operation it swarmed with life, and was covered with a fatty scum formed of matted *Bacteria*.

Many similar experiments were subsequently made. On the 27th of January, for example, six bulbs of turnip-infusion were boiled for 220 minutes, six for 300 minutes, and two for 305 minutes. Suspended in the air above each infusion was a sprig of old Colchester hay, this being purposely introduced to augment the chance of infection. Notwithstanding its presence the bulbs were one and all permanently sterilized. The specific gravity of the infusion was in all cases 1007.

The sprigs of old hay were afterwards shaken into the liquid, but they produced no effect. For weeks afterwards the infusion remained clear. Was this impotence to generate life due to the fact that the nutritive power of the infusion had been destroyed by the "blighting influence of heat?" Not so; for when the same infusion was infected by a sprig of fresh hay. by a small pellet of cotton-wool rubbed against the dusty shelves of the warm room, or by a speck of another infusion containing *Bacteria*, it never failed to develop life. The only observed difference between the effect produced by the dry hay or dust and the living *Bacteria* was purely a difference of time. Inoculation with the finished organisms acted more rapidly than infection with the dust, but the effects were the same in the end.

On the 27th of January also nine melon-bulbs were treated exactly like the turnip, being furnished with sprigs of old Colchester hay, plugged with cotton-wool, and hermetically sealed above the plugs. Six of them were boiled for 215 minutes, and three for 220 minutes. They were one and all permanently sterilized; but, like the turnip, all of them were open to infection by fresh hay, dry dust, or living *Bacteria*. The specific gravity of the melon-infusion was 1008.

§ 17. *Further Experiments on the Resistance-limit of Germs to the Boiling Temperature.*

The amount of boiling which turnip-infusion failed to withstand is shown by some of the foregoing experiments; but to determine the limit of its resistance we must begin with shorter periods. On the 1st of March, therefore, eight groups of pipette-bulbs were charged with turnip-infusion which had been prepared in an atmosphere purposely infected with the germs of old Heathfield hay. In every case, moreover, a sprig of the same hay was placed in the air above the infusion. The bulbs had their necks plugged with cotton-wool, and were hermetically sealed above the plug. In this condition the respective groups were boiled for the following times:—

1st group	15 minutes.
2nd "	30 "
3rd "	45 "
4th "	60 "
5th "	75 "
6th "	90 "
7th "	105 "
8th "	120 "

After boiling they were removed, permitted to cool, had their necks broken off by a file, and were afterwards exposed to the temperature of our warm room. It may be remarked that the infusion gradually deepened in colour from the 15-minute period, where the colouring was hardly sensible, to the 2-hour period, where the colouring became deep yellow. The effect was doubtless due to the oxidation of the infusion, which, notwithstanding the colour, was in all cases highly transparent.

Two days after their preparation, every tube of the series had become turbid and had begun to cover itself with scum.

On March the 6th the periods of boiling were prolonged with a fresh infusion. Two groups of tubes were, on that day, exposed to boiling water for the following times:—

1st group	180 minutes.
2nd "	240 "

On the 8th of March all the members of the first group were turbid and covered with scum. The second group was completely sterilized. This latter result is quite in accordance with the experiments made on the 27th of January. Turnip-infusion was then boiled for periods varying from 220 minutes to 305 minutes, complete sterilization being in all cases the consequence. These results were subsequently checked by a continuous series of experiments extending over periods of boiling varying from one to six hours. Up to three hours the infusion resisted sterilization; but when the periods of boiling were prolonged to four, five, and six hours respectively, all the bulbs became permanently barren. The liquid continued in the highest degree transparent, and in colour a brilliant orange-brown.

Experiments intended to determine the limit of resistance of cucumber-infusion were made on the 24th of February. Nine pipette-bulbs were then charged, plugged, hermetically sealed, and subjected to the boiling temperature for the following times:—

1st bulb	15 minutes.
2nd ,,	30 ,,
3rd ,,	45 ,,
4th ,,	60 ,,
5th ,,	120 ,,
6th ,,	180 ,,
7th ,,	240 ,,
8th ,,	300 ,,
9th ,,	360 ,,

After boiling and cooling they had as usual their ends broken off by a file. The result here was that at the 5th bulb, which corresponded to a boiling for two hours, the life-development suddenly ceased. All the tubes boiled from three to six hours inclusive were completely sterilized.

The infusion in this case had been diluted by an accident, so that its specific gravity was hardly above that of distilled water. On the 28th of February, therefore, a fresh infusion having a specific gravity of 1006 was prepared, and introduced into a series of bulbs exactly as in last experiment. The bulbs were exposed to the boiling temperature for the following times:—

1st bulb	15 minutes.
2nd ,,	30 ,,
3rd ,,	45 ,,
4th ,,	60 ,,
5th ,,	120 ,,
6th ,,	180 ,,
7th ,,	240 ,,
8th ,,	300 ,,
9th ,,	360 ,,

The result here was that at the 6th bulb, which corresponded to three hours' boiling, the life-development suddenly ceased. All the bulbs boiled from 15 minutes to 180 minutes inclusive proved fruitful; while from 240 minutes to 360 inclusive all were completely sterilized. As in the case of the turnip-infusion, the cucumber subjected to long periods of boiling assumed an orange-brown tinge.

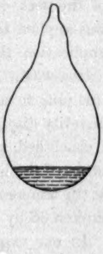
Comparing these results with those obtained with the turnip-infusion, it will be observed that cucumber and turnip exhibit about the same resistant power: three hours' boiling, and less, failed to sterilize both of them; four hours' boiling, and more, rendered both of them permanently barren.

The cucumber-infusions prepared on the 22nd and 28th of February were connected with the atmosphere through the cotton-wool plugs; but no attempt had been made to remove its floating matter from the air above the infusions. On the 22nd, however, four bulbs of the infusion were also prepared, charged with filtered air, left unplugged, and hermetically sealed. The same was done with four bulbs on the 28th of February. Each group was subjected to periods of boiling of 15, 30, 45, and 60 minutes respectively. All of them became turbid; but it was interesting to notice the gradual and obvious fall of life from the 15-minute to the 60-minute period. Could the *Bacteria* have been counted, and the result graphically represented, the ordinate corresponding to the abscissa 15 would have been found very considerably longer than that corresponding to the abscissa 60.

The method of experiment here for the most part pursued was employed by SPALLANZANI and NEEDHAM. It was afterwards extensively applied by the late excellent Professor WYMAN, of Harvard College, while in 1874 it was materially refined and improved upon by Dr. WILLIAM ROBERTS of Manchester. The method is hampered by one grave doubt. The air, plus its floating matter, is imprisoned in the sealed bulbs, so that the heat applied has not only to destroy the germs clasped by the infusion, but also those diffused through the supernatant atmosphere. Now it is not certain whether an amount of heat which would be absolutely destructive to a germ embraced by a hot liquid may not be wholly ineffectual when acting on a germ floating in vapour or air. Throughout SPALLANZANI'S and NEEDHAM'S experiments, throughout those of WYMAN and ROBERTS, and throughout my own, as reported in this section and the last, this possibility of error runs. Such experiments, in short, do not enable us to state with certainty the temperature at which an infusion is sterilized, because the germs which most pertinaciously oppose sterilization may not belong to the infusion at all, but to the adjacent air.

The most astonishing cases of resistance to sterilization observed by WYMAN were associated with this particular mode of experiment. The possible action of the uncleansed air, moreover, was in his case augmented by the fact that he employed quantities of liquid, very small in comparison with the size of his flasks. In some of his earlier experiments the volume of air was more than thirty times that of the infusion. These relative volumes are represented in the annexed figure (fig. 7), copied from WYMAN'S Memoir of 1862[*].

Fig. 7.



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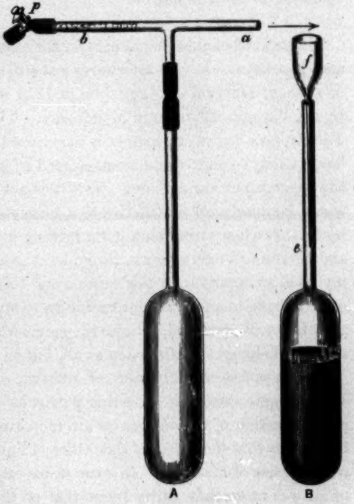
[*] SILLIMAN'S American Journal, vol. xxiv. p. 80.

§ 18. *Change of Apparatus. New Experiments with Filtered Air.*

The source of possible error referred to in the last section had been long present to my mind, and I had already taken measures to avoid it. On the 2nd of January, 1877, an infusion of turnip (sp. gr. 1006) and an infusion of melon (sp. gr. 1008) were prepared and introduced into a series of pipette-bulbs in the following manner:—One end *a*, fig. 8, of a glass T-tube was connected with an air-pump, the other end *b* was closely plugged with cotton-wool, while to the third branch of the T-tube the neck of the pipette-bulb A was attached by india-rubber tubing. A piece of the same tubing, furnished with a pinchcock *p*, was also attached to the free end of the T-tube beyond the cotton-wool.

The bulb A was exhausted three times in succession, the pinchcock *p* being closed, and was three times filled with filtered air, the pinchcock being opened. At the third exhaustion the bulb was raised to a very high temperature by a Bunsen flame, and finally filled with filtered air. It was then plunged for a minute into ice-cold water, from which it was afterwards removed, detached from the T-tube, and then charged with the infusion by means of a narrow pipette, *f e*, shown at the top of B, fig. 8.

Fig. 8.



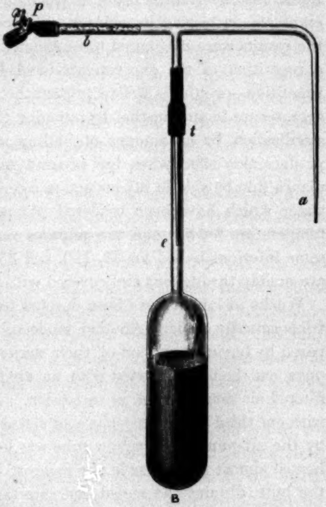
The rationale of the above proceeding is this:—On quitting the ice-cold water for the warmer air of the laboratory, expansion of the air within the bulb would occur. This would cause a gentle motion from within outwards, opposing all indraught of contaminated air. The entry of the infusion into the bulb would, I thought, also promote this outward motion. On the removal of the pipette, which occupied but a very small portion of the neck of the bulb, a little warmth was applied to the latter, and during its application the neck was plugged with cotton-wool. The air entering through this plug to supply the place of the small quantity displaced by the warmth would, I concluded, reach the interior of the bulb perfectly sifted of its floating matter. The necks of the bulbs were hermetically sealed, and the infusions maintained for ten minutes at the temperature of boiling water. After a lapse of twelve hours their sealed ends were broken off by means of a file.

In our experiments on the 28th of December turnip and melon subjected to ten minutes' boiling entirely gave way. In these present experiments, where greater care was taken, two out of the six turnip-bulbs and three out of the six melon-bulbs remained permanently barren. Even this amount of success proved afterwards so exceptional that it might be fairly regarded as accidental.

On the 4th of January the experiments were continued. The pipette-bulbs employed were first carefully washed with carbolic acid, which was removed as far as possible with ordinary water. They were then washed with a solution of caustic potash, and finally rinsed out with distilled water. They were not subjected to the action of the Bunsen flame. The infusions employed were turnip (sp. gr. 1006) and melon (sp. gr. 1008), eight bulbs being filled with each infusion.

I could not be certain that the motion of the liquid fillet at the end *e* of the pipette with which the bulb B, fig. 8, had been charged, had not drawn into the neck of the bulb a modicum of the external air. In the present experiments, therefore, the method of charging the bulbs was modified in the following way:—The glass T-tube employed in our last experiments had its end *a*, which was to be connected with the air-pump, drawn out to a small orifice and bent as in fig. 9. The branch connected with the bulb was also drawn out to a tube of fine bore, which entered the neck of the bulb for some distance to *e*, the thicker part above being connected with the neck of the bulb by india-rubber tubing. The end *b*, as before, was plugged with cotton-wool and provided with a pinchcock, *p*. The object here aimed at was that the liquid should be discharged into the bulb far below the india-rubber connecting-piece, and that during the discharge it should pass only through filtered air.

Fig. 9.



Each bulb was exhausted in the manner already described, and refilled three times in succession. When last filled it was plunged for a minute or so into iced water, with the view of rendering the air within the bulb denser than that without. The pinchcock *p* being closed, the whole apparatus was then detached from the air-pump. On being lifted from the iced water into the warmer air there was a gentle outflow of air from *a*.

The mode of charging the bulb was this:—The point *a* was well sunk into the infusion, and the associated bulb, B, was plunged into boiling water. There was an immediate outrush of air from *a* which bubbled through the liquid. As soon as the bubbling had relaxed a little, *a* being still submerged, the bulb was transferred to iced water. A shrinking of the warm air was the consequence, and through *a* the infusion was forced by atmospheric pressure. It descended the middle branch of the T-tube, and was discharged from its end *e* into the bulb. The quantity of liquid obtained by a first immersion in the iced water was not sufficient to charge the bulb; but by repeating the process of heating and chilling two or three times, the point *a* never being permitted to quit the infusion, any required quantity was with ease and accuracy introduced. The neck of the bulb was finally detached from the T-piece by loosening the india-rubber tube *t*. The bulb was then slightly warmed so as to cause an outflow from within, and while this outflow continued the neck was plugged with cotton-wool. It was sealed above the plug, and after the cooling of the infusion the sealed end was broken off with a file.

It is not my intention to take up the Society's time in describing in detail the numerous experiments made in accordance with this method, or the variety of infusions employed in testing its efficacy. Suffice it to say that, notwithstanding all my care, the results were chequered throughout. Sometimes success would seem complete, but a repetition of the experiments—and I never felt safe without frequent and varied repetition—would, as before, present the success in the light of an accident. I am, however, secure in stating that by pursuing this plan I have in some cases effected complete sterilization by an amount of boiling which, twenty times multiplied, has failed to produce this effect when less accurate methods were resorted to. I have for example placed side by side in my collection a series of organic infusions as pellucid as distilled water which have been rendered permanently sterile by an exposure to the boiling temperature for five and ten minutes respectively, and a second series containing the same infusions boiled for 30, 120, and 330 minutes respectively, and which nevertheless are muddy throughout and covered with scum.

Weeks of labour have been devoted to these experiments, nor did they exhaust the trials actually made. Another mode of proceeding was this. Pipette-bulbs were prepared by having a portion of their necks drawn out to a tube of very fine bore. The open end being connected with an air-pump, the bulb was exhausted and filled with filtered air several times in succession. In the final experiment the bulb was charged with one third of an atmosphere of cleansed air; and while this pressure was maintained by the air-pump the narrow tube was hermetically sealed. Each bulb was afterwards heated almost to redness in the flame of a Bunsen lamp. It was charged by inverting the bulb, dipping the sealed end into the infusion, and breaking it off underneath the surface. The liquid entered until the bulb was two thirds filled, when the narrow tube was again sealed without permitting its open end to quit the infusion. A great number of experiments were thus executed, the results of which distinctly favoured the conclusion, though they did not to my satisfaction prove it, that the resistant germs were not to be wholly ascribed to the air, but that they had survived in the liquid.

§ 19. *Final proof that the Resistant Germs are embraced by the Infusion. Examples of Resistance both in Acid and Neutral Liquids.*

I have here touched upon the question which chiefly harassed me at the time to which I now refer. It was this:—Have the germs, which under the circumstances here described produced life, been really embraced by the infusion itself during the time of heating? The liquid, it will be remembered, had to pass through the neck of the bulb, and it could not descend from the neck into the bulb without leaving a film adherent to the internal surface of the neck. This film, I reflected, might dry in part by evaporation; it might, in doing so, leave germs behind which would be very differently circumstanced from those in the liquid. To germs thus exposed, not to the heat of water, but to the possibly less effective heat of vapour and air, the observed life might I thought be due. Before closing definitely with the proposition that the surviving germs had actually been in the liquid, the possibility to which I have just referred had to be shut out.

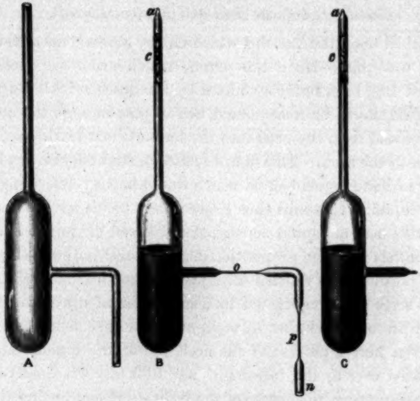
The evil was to some extent mitigated by a suggestion of my assistant to charge the bulb, not through its own neck, but through a narrow tube issuing at right angles to the neck. But even here a portion of the neck and of the higher interior surface of the bulb was trickled over by the infusion. The difficulty was finally met by causing the lateral tube to issue from the centre of the bulb itself, and forcing the infusion into the bulb by atmospheric pressure, until the surface of the liquid stood clearly above the lateral orifice. To this level the liquid rose without wetting any portion of the surface against which it did not permanently rest.

The precise method pursued in preparing and charging the bulbs was this:—First, the bulb as sent to us by the glass-blower is represented at A, fig. 10. Its neck is first plugged with cotton-wool (*c*) and hermetically sealed as at B, fig. 10. The lateral tube is then drawn out to almost capillary narrowness at *o* and *p*. The end *n* is connected with an air-pump, by which the bulb is exhausted, and after two or three emptyings and fillings, it is finally charged with one third of an atmosphere of thoroughly filtered air. While the pump attached to *n* maintains this pressure within the bulb, the capillary tube *p* is sealed with a lamp. The bulb and its appendages are then heated nearly to redness in a Bunsen flame, all life adherent to the interior surface being thus destroyed.

The end *p* is then introduced into the infusion, pressed against the bottom of the vessel that contains it, and thus broken. The external pressure of a whole atmosphere, having but one third of an atmosphere within the bulb to oppose it, forces the liquid through the lateral tube. It enters the bulb, gradually rising until it reaches the orifice, and rises above it. When the pressure within is exactly equal to the pressure without, two thirds of the bulb are occupied by the liquid.

The infusion then extends without breach of continuity from the bulb B to the vessel in which the end *p* is immersed, the uncleansed air being thus completely excluded. A small gas-flame is carefully applied at *o*. The liquid within the narrow tube vaporizes, and the vapour drives the liquid to some distance right and left from the place of

Fig. 10.



heating. In the absence of the liquid the fine tube reddens, fuses, and is hermetically sealed. The aspect of the bulb after it has been thus charged is shown at C, fig. 10.

By this method, on the 20th of February sixteen bulbs were charged with infusions of old Heathfield hay and of a hard wiry hay from Guildford, not old. They were divided into four groups, four bulbs in a group. Each group embraced two acid and two neutral infusions. They were boiled for the following times:—

1st group	10 minutes.
2nd ,,	20 ,,
3rd ,,	30 ,,
4th ,,	60 ,,

After the bulbs had sufficiently cooled, their sealed ends were removed by a file.

On the 21st of February, less than twenty-four hours after their preparation, all these bulbs showed signs of yielding. On the 22nd they were all turbid, while, as regards the comparison of acid and neutral infusions, their condition was this:—

1st group.	10 minutes.	Guildford neutral distinctly more turbid than Guildford acid. Scum on former, none on latter. Old Heathfield neutral not to be distinguished from old Heathfield acid. Both turbid and covered with scum. Much lightened in colour.
2nd group.	20 minutes.	Guildford neutral distinctly more turbid than the acid liquid. Old Heathfield neutral more turbid than the acid infusion.

3rd group.	30 minutes.	Guildford neutral a little more turbid than Guildford acid: difference small. Old Heathfield neutral more turbid than the acid infusion; the former with scum, the latter none.
4th group.	60 minutes.	Guildford neutral distinctly more turbid than Guildford acid; the former with scum, the latter free from scum. Old Heathfield neutral somewhat more turbid and scummy than the acid liquid: difference not great.

Here I take it to be absolutely certain that the germs which resisted sterilization were contained in the liquid. The maximum period of boiling was 60 minutes.

On the 22nd of February four groups of bulbs were charged as above described with the same two infusions, and the periods of boiling prolonged as follows:—

1st group	. . . .	90 minutes.
2nd ,,	. . . .	120 ,,
3rd ,,	. . . .	180 ,,
4th ,,	. . . .	240 ,,

As in the last case, neutral and acid infusions of each kind of hay were operated on. This was the result:—On the evening of the 23rd, that is to say, 24 hours after their preparation, every one of these bulbs disclosed to the practised eye that organisms existed within it. At 2 P.M. on the 24th they all swarmed with life. The old Heathfield bulbs, both acid and neutral, were turbid and covered with scum, the very weakly-acid infusions being indistinguishable in appearance from the neutral ones. In the case of the Guildford infusion, however, the scum on the neutral infusions was richer and heavier than that on the acid ones.

Four hours mark the limit to which the boiling was carried in these experiments. On the 27th of February the periods of boiling were still further prolonged. Four groups of bulbs charged with infusions of the same two kinds of hay, both acid and neutral, were on that day boiled for the following times:—

1st group	. . . .	300 minutes.
2nd ,,	. . . .	360 ,,
3rd ,,	. . . .	420 ,,
4th ,,	. . . .	480 ,,

I had previously boiled infusions of old Heathfield, old London, and old Colchester hay for 5 hours, and found them afterwards permanently barren. In the present instance, also, all the bulbs boiled for 5 hours, 6 hours, and 7 hours were completely sterilized. They remained ever afterwards perfectly brilliant. This, with one exception, was also the deportment of the group of bulbs boiled for 8 hours. The exception was a neutralized bulb of Guildford infusion, which became turbid and covered with scum. Considering the severity with which the bulb had been treated prior to charging, and considering the mode of charging it, I do not think that the life developed could have been the product of external germs. Through profound and thorough hardening and desiccation, through defect of contact with the liquid or some other cause, some germs in the infusion itself had, I doubt not, been enabled to withstand the extraordinary ordeal here described.

Was it "the blighting influence of heat" that deprived these 8-hour bulbs of the power of spontaneous generation? Whatever be the meaning attached to such language the reply is obvious, that the "blighting" was the same for all the bulbs; and yet we find one of them, which, when taken from the boiling water, was perfectly brilliant, rendered in two days muddy with organisms. Further, it was only necessary to wash with perfectly sterilized distilled water the adherent germs from a small bunch of hay, and to inoculate the clear infusion in an 8-hour bulb with the washing-water, to cause it within four and twenty hours to become turbid throughout. To speak more definitely, 14 hours in the warm room were found sufficient to cloud the infected 8-hour bulb with *Bacteria*. Thus the infusion, when living germs are restored to it, shows its perfect competence to develop them, and it was solely the destruction of the germs which it possessed before it was boiled that rendered it sterile afterwards.

It is worth bearing in mind that the particular kinds of hay whose germs manifested these extraordinary powers of resistance were so sapless and indurated that the specific gravity of their infusions, even after five hours' digesting, could not without difficulty be sensibly raised above that of distilled water. This was more especially the case with old Heathfield and old Colchester hay, the infusions of which, though highly coloured, were marked, almost without exception, "specific gravity 1000." Old London hay-infusion was usually 1003, while infusions of new Heathfield hay were raised without difficulty to 1007*.

As already stated, I never felt safe in these experiments until they were checked by careful repetition. A partial corroboration has been already adverted to. But on the 2nd of March I had the same infusions prepared, kept in a cool place throughout the night, and introduced into four groups of bulbs next morning. The infusions were all brilliant. During the process of boiling one group of bulbs blew up, hence one link is absent from the series. I thought the 4-hour group could be best spared, and therefore selected it for omission. We had thus the infusions subjected for one, two, three, five, and six hours respectively to the boiling temperature. The three first hours agree exactly with their predecessors. No single bulb within these limits was sterilized, all of them became turbid throughout and loaded with scum. One bulb of the 5-hour group and one of the 6-hour group also became turbid and flocculent within, but without any scum upon the surface. As in the case of the 8-hour bulb already mentioned, this appearance of life was, I doubt not, due to stray germs of exceptional resisting power, which maintained themselves unscathed in the infusions after their fellows had been destroyed.

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* A comparative experiment on dried and undried peas, described in the 'Proceedings' of the Royal Society (1877, vol. xxv. p. 503), may be referred to here as illustrating the manner in which desiccation restricts diffusion, and thus tends to preserve the integrity of the desiccated germ. I suppose the original mineral salts of the hay were still retained in the old samples; but hot water appeared to have little power of extracting them. The resistance of the hay in this respect appeared to be shared by its germs.

By multiplied experiments of a similar character executed subsequently, and fortified by others made in a different way, all doubts as to the real ordeal to which the germs had been exposed were set at rest. A flood of light, moreover, was thrown upon the difficulties recorded in the foregoing pages. Prior to the introduction into our laboratory of the particular samples of desiccated hay whose adherent germs had manifested such extraordinary powers of resistance, infusions of all kinds, even those of hay itself, were sterilized with ease and certainty. But the old London, the old Heathfield, the Guildford, and the old Colchester hay brought a plague into our atmosphere, and thus the infusions of other substances, some samples of hay included, became the victims of a pest entirely foreign to themselves. The failure to sterilize cucumber, turnip, beetroot, artichoke, melon, beef, mutton, haddock, herring, sole, was plainly due to the fact that their infusions had been prepared in an atmosphere[*], or brought into contact with vessels, contaminated with germs which have been here shown capable of resisting 240 minutes' boiling. It is obvious from all this that to speak of an infusion being rendered barren by such or such a temperature, is simply to use words without definite meaning; because the temperature at which any infusion is sterilized depends upon the character and condition of the germs which find access to it. The death-temperature, for example, may be more than three hours in London and less than three minutes at Kew[*].

I may cite here two conspicuous illustrations of the infective energy of those desiccated hay-germs in two infusions which, under ordinary atmospheric conditions, are very easily sterilized. On the 30th of March five pipette-bulbs were charged with clear beef-infusion and boiled for the following times:—

1st bulb	60	minutes.
2nd „	120	„
3rd „	180	„
4th „	240	„
5th „	300	„

After cooling, the sealed ends were broken off, the air being admitted through cotton-wool plugs. Every one of these bulbs became charged with organisms. In the shed, eight yards off, this beef-infusion was, as already reported, sterilized by five minutes' boiling.

Precisely the same experiment was made on March 30 with pellucid mutton-infusion. Not one of the bulbs was sterilized. All of them are at this moment charged with life.

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[*] I have already described the distribution of *Bacteria*-germs in the air as "Bacterial clouds." Were our vision sufficiently sharpened to see the manner in which such germs are distributed over the surface of a meadow, we should not, I am persuaded, find that distribution uniform. We should, in my opinion, find the germs grouped in crowds, with comparatively free interspaces, like violets on an alp, or mushrooms in a field. It is therefore conceivable that two bunches of hay from the same meadow may differ from each other in deportment.

It behoves those engaged in the industry of preserved meat and vegetables to keep clear of the old-hay contamination. Probably they from time to time have encountered difficulties and disappointments which they could not explain, but which may be solved by reference to the results here set forth. But above all the practical question arises:—May not the surgeon have to fight sometimes against enemies like those here described? The particular germs with which I have been so long contending cause, as we have seen, both fish and flesh to putrify. How would they behave in the wards of an hospital? Can they cause wounds to putrify? and if so, would they succumb to the disinfectants usually applied? These are questions the weighty import of which will be best understood by the enlightened follower of the antiseptic system, and which he will know how to answer for himself.

§ 20. *Remarks on Acid, Neutral, and Alkaline Infusions.*

In the foregoing section reference was made to the comparative deportment of acid and neutral infusions. There can be no doubt of the fact that, for the nutrition and multiplication of *Bacteria*, acid infusions are less suitable than neutral or slightly alkaline ones. In acid infusions exposure to the common air sometimes copiously develops *Penicillium*, while it fails to develop *Bacteria*. It is also true that exposure for a certain time to a certain temperature may in many cases prevent the appearance of life in an acid infusion, and fail to prevent it in a neutral or slightly alkaline one.

In the present inquiry this has been frequently found to be the case. I have many closed chambers, for example, to which the process of "discontinuous heating," to be subsequently described, has been applied; and with them it has proved a common experience that an amount of heating which has rendered acid infusions of hay permanently barren has failed to sterilize the corresponding neutral infusions. Moreover, in the cases just recorded, where single bulbs escaped sterilization though exposed for five, six, and eight hours to the boiling temperature, it was always a neutral bulb that kindled into life. To these instances another may be added here. On the 22nd of March an infusion of the wiry Guildford hay already referred to was divided into two parts, one of which was neutralized and the other left acid. Five pipette-bulbs were filled with the one infusion and five with the other. After hermetic sealing they were all completely submerged in water and boiled for six hours. Every one of the acid bulbs was sterilized by this process, while in two days three of the five neutral ones became turbid and covered with scum.

The best thought that I have been able to bestow upon this subject does not induce me to lean towards the explanation suggested by M. Pasteur, namely, that the germs escape the destructive action of the heat because they are not wetted by the alkaline or neutral liquid. From the comparative action of alkalized and acidulated water upon hay, I should be inclined to infer that the wetting of its germs by the former would be more prompt than by the latter. The question, I think, is not one of wetting, but of relative nutritive power. Two *Bacteria*-germs of equal vital vigour dropping from the atmosphere, the one into a neutral or slightly alkaline, the other into an acid infusion, soon cease to be equal in vigour. The life of the one is promoted, the life of the other only tolerated by its environment. When the temperature surrounding both is raised to a prejudicial height the one will suffer more than the other, because equally inclement conditions are brought to bear upon constitutions of different strengths; and if the temperature be sufficiently exalted or sufficiently prolonged to become fatal, the more weakly organism will be the first to give way. A germ, moreover, brought close to the death-point in a neutral or an alkaline infusion may revive, while in an acid one it may perish—just as proper nutriment may rescue a dying man while improper nutriment would fail to do so. These elementary considerations, founded on the demonstrable fact that *Bacteria*-germs are more fully vivified and better nourished in neutral infusions than in acid ones, suffice, I think, to explain the observed difference of action. At all events, these are the thoughts which have become rooted in my mind, through long observation and long pondering of this question*.

§ 21. *Remarks on the Germs of* Bacteria *as distinguished from* Bacteria *themselves.*

The failure to distinguish between these stubborn germs and the soft and sensitive organisms which spring from them has been a fruitful source of error in writings on Biogenesis. In his able and important paper, "On the Origin and Distribution of *Bacteria* in Water, and the circumstances which determine their existence in the Tissues and Liquids of the Living Body," Dr. Burdon Sanderson, for example, has described experiments from which, in my opinion, very incorrect conclusions have been drawn. He exposed to the common air vessels containing Pasteur's solution, which when inoculated with fully developed *Bacteria* enables them freely and copiously to increase and multiply; he even caused the air to bubble through the solution, and finding that though *Torula* and *Penicillium* were luxuriantly developed in the liquid, *Bacteria* never made their appearance, he concluded, "not merely that the conditions of origin and growth of *Bacteria* and fungi are considerably different, but that, as regards the former, the germinal matter from which they spring *does not exist in ordinary air*"†. These italics occur in the paper from which I quote.

Dr. Sanderson subsequently reaffirms the position here laid down. "In my preceding experiments," he says, "it has been shown that although *Torula*-cells and *Penicillium* appear invariably, and without exception, on all nutritive liquids of which the surfaces are exposed to the air, without reference to their mode of preparation, no amount of exposure has any effect in determining the evolution of *Bacteria*"‡. And, again, with reference to another experiment:—"The result shows that ordinary air is entirely free from living *Bacteria*"[*]. His general conclusion is that, as regards the development of *Bacteria* in organic liquids, "water is the contaminating agent."

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* From their deportment in boiling, I should infer that the air dissolved in an alkaline liquid is in a different physical condition from that dissolved in an acid liquid; and to this, in some measure, the difference of nutritive power may be due. I have been unable to find any experiments on the comparative absorption of air by acid and neutral liquids. The subject is, I think, well deserving of attention.

† Appendix to the Thirteenth Report to the Medical Officer of the Privy Council for 1871, p. 335.

‡ 'Appendix,' p. 338. Though Dr. Sanderson speaks of "all nutritive liquids," if I understand him aright, he really tried but one, and that was a mineral solution, not an animal or vegetable infusion.

Upon these experiments, and the conclusion drawn from them, an argument has been founded by Dr. BASTIAN[†], which would be weighty were its basis sure. In reference to the Presidential Address of the British Association in Liverpool[‡], he argues thus:—"Speaking of living *Bacteria* germs, Professor HUXLEY summed up by saying, 'considering their lightness, and the wide diffusion of the organisms which produce them, it is impossible to conceive that they should not be suspended in the atmosphere in myriads.' Had Profesor HUXLEY himself made some careful and discriminating experiments on this part of the subject, he might have found that the supposed impossibility of conception was entirely delusive. . . . . What has been the subsequent progress of events? In the first place, it has been shown by Professor BURDON SANDERSON, myself, and others, that the living *Bacteria* germs are not diffused through the air to any appreciable extent; and this is now a very widely accepted doctrine, in spite of its being, as Professor HUXLEY imagined, an impossible conception." The "others" referred to by Dr. BASTIAN embrace among them, it is to be admitted, the celebrated naturalist COHN.

Dr. BASTIAN was quite correct in saying that the "doctrine" he enunciated was, at that time, "widely accepted." It is, nevertheless, one of those cases in which the general acceptance of a doctrine fails to establish its truth. It is, indeed, an entirely erroneous doctrine, founded, I will not say upon incorrect experiments, but on the misinterpretation of incomplete ones. I have already referred to this error, and should not do so now had it not been lately revived. The deportment of almost any sterilized animal or vegetable infusion exposed to common air will disprove the doctrine. It has been disproved repeatedly by experiments with melon-, turnip-, cucumber-, and hay-infusions, alluded to in this memoir. Such infusions, after having been sterilized by exposure for six or eight hours to the boiling-temperature, remain, if protected from the *Bacteria*-germs of the air, for ever barren; but when infected spontaneously, or purposely, by atmospheric germs, they are found, within eight and forty hours after such infection, thickly crowded with *Bacteria*. That London air is laden with living *Bacteria* germs is as certain as that London chimneys are laden with smoke. What Dr. SANDERSON's important experiments really prove is, that a mineral solution competent to nourish the *Bacteria* after they have been fully developed is not competent (or, rather, but very feebly competent) to effect the transfer from the germ state to that of the finished organism. It can feed the chick, but it cannot hatch the egg. As I have already expressed it, the experiment proves, not the absence of *Bacteria*-germs from the air, but the inability of the mineral solution to develop them.

Another experiment, described by Dr. SANDERSON in the paper above referred to, is this:—"A glass rod was charged with *Bacteria* by dipping it into a solution on the surface of which there was a viscous scum, consisting entirely of these bodies imbedded in a gelatinous matrix. The rod was allowed to dry in the air for a few days. It was then introduced into boiled test-solution contained in a superheated glass. On February 6th the liquid was already milky, and teemed with *Bacteria*.

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[*] 'Appendix,' p. 339.

[†] 'Evolution,' p. 44.

[‡] Brit. Assoc. Report, 1870.

"To determine the effect of more complete desiccation, an éprouvette containing one cubic centimeter of cold water, previously ascertained to be zymotic, was evaporated to dryness in the incubator, and kept for some days at a temperature of 40° Cent. On February 20th the dried glass was charged with boiled and cooled solution, and plugged with cotton-wool in the usual way. The liquid was examined microscopically on March 2nd, when it contained numerous *Torula*-cells, but no trace of *Bacteria*. It therefore appears that the germinal particles of *Bacteria* are rendered inactive by drying without the application of heat.

"As it appeared probable," continues Dr. SANDERSON, "that in the previous experiments with *Bacteria*-scum desiccation might be prevented by the gelatinous matrix, a portion of the same scum was thoroughly washed with water, collected in the éprouvette, and dried for some days in the incubator. The éprouvette was then charged with both boiled and cooled PASTEUR's solution, and plugged with cotton-wool. On March 11th the liquid was slightly hazy, but was found to contain no trace of *Bacteria*. The haziness was due to the presence of *Torula*-cells in great numbers. It thus appeared that fully-formed *Bacteria* are deprived of their power of further development by thorough desiccation."

To the present hour these experiments are quoted as conclusive in reference to the influence of desiccation. They are quoted, moreover, as applicable not only to the developed *Bacterium*, but also, without restriction, to the germs from which *Bacteria* spring. "To maintain," says Dr. BASTIAN [*], "his Panspermism in the face of his own experiments, SPALLANZANI was compelled to assume that the germs of the lower infusoria do possess this seed-like property of developing after desiccation. Modern science, however, declares that they have no such property. We are told most unreservedly by Professor BURDON SANDERSON, not only that the germinal particles of *Bacteria* are rendered inactive by thorough drying, without the application of heat, *i.e.* by mere exposure to air for two or three days at a temperature of 104° Fahr., but also that fully formed *Bacteria* are deprived of their power of further development by thorough desiccation." In this unqualified sense the conclusion is certainly an erroneous one; and that it is so has been proved by experiments far more stringent than those of Dr. SANDERSON. I could cite a multitude of such experiments, but a reference to one or two of them will here suffice.

A small bunch of old Heathfield hay was washed with distilled water, which was received into a champagne-glass. The glass was placed on a stove until the water had all evaporated, and the dried residue was permitted to remain upon the stove for several days. Dr. SANDERSON's drying temperature was 104° Fahr., mine was 120° Fahr., and my period of drying was longer than his. Scraping a little of the dry sediment treated as above from the bottom of the champagne-glass, I infected with it a bulb containing hay-infusion which had been completely sterilized by eight hours' boiling. When infected, the infusion was brightly transparent, but forty-eight hours after its infection it was teeming with *Bacteria*. With regard to the doctrine that these organisms arise from "dead organic particles," instead of from living germs[*], no scientific man at the present day ought, I submit, to be called upon to spend a moment's thought upon it.

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[*] 'Evolution,' p. 156.

One other reference will suffice. I have had bundles of hay hung up for seven or eight weeks in the hot rooms of the Turkish Bath in Jermyn Street, and exposed during the whole of this time to a temperature of 140° and upwards. The germs adherent to this hay were not killed by even this amount of desiccation. When a sterilized animal or vegetable infusion was infected with them they gave birth in the usual time to swarms of *Bacteria*.

§ 22. *Sterilization by discontinuous Heating.*

Keeping the distinction between germs and developed organisms here insisted on, and the probable changes that occur in passing from the one to the other, clearly in view, I have been able to sterilize with infallible certainty the most obstinate infusions referred to in this paper; and this has been accomplished without either raising the temperature of the infusions beyond their ordinary boiling-point, or inordinately prolonging the application of heat. The infusions may be sterilized by a temperature even below that of boiling water, while the time of its application may be but a minute fraction of that resorted to in some of the foregoing experiments.

It is an undisputed fact that active *Bacteria* are killed by a temperature far below that of boiling water. It is also a fact that a certain period, which I have called the period of latency, is necessary to enable the hardy and resistant germ to pass into that organic condition in which it is so sensitive to heat. There can hardly be a doubt that the nearer the germ approaches the moment when it is to emerge as the finished organism, the more susceptible it is to that influence by which that organism is so readily destroyed. We may learn from experience, aided by the power of search which the concentrated luminous beam places in our hands, what is the approximate time required for the Bacterial germ to pass into the *Bacterium*. Say that it is twenty-four hours. Supposing the heat of boiling water, or even a lower heat than that of boiling water, to be applied to the germ immediately before its final development, when all its parts are plastic, when it is, in short, on the point of reaching a stage at which a temperature of 140° Fahr. is demonstrably fatal. It is in the highest degree probable that a temperature of 212°, or of 200°, or, indeed, a temperature of 150° if applied sufficiently often or for a sufficient length of time, will prove fatal to the germ, and prevent the appearance of the still more sensitive organism to which the germ is on the point of giving birth.

Here, at all events, we have a theoretic finger-post pointing out a course which experiment may profitably pursue. It is not to be expected that the germs with which our infusions are charged all reach their final development at the same moment. Some are drier and harder than others, and some, therefore, will be rendered plastic and sensitive to heat before others. Hence the following procedure.

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[*] 'Evolution,' p. 44.

Four and twenty small retorts were charged with hay-infusions on the 1st of February, and subjected morning and evening to the boiling temperature for one minute. The last heating took place on the evening of the 3rd of February. The retort-necks had been plugged with cotton-wool; the air within them, however, had not been filtered, and there was comparatively little care bestowed on their preparation. After the final heating they were abandoned to the temperature of a room kept close to 90° Fahr. A series of similar retorts charged at the same time with the same infusions were boiled continuously for ten minutes, plugged while boiling, and placed in the same warm room. Two days after their preparation the retorts last mentioned had, without a single exception, given way to turbidity and scum. On the other hand, twelve of the twenty-four retorts which had been subjected for a much shorter period to the discontinuous boiling remained permanently brilliant and free from scum.

On the 1st of February eight pipette-bulbs were charged with two hay-infusions, four bulbs being devoted to each. The air above the infusions was the unfiltered air of the laboratory. They were subjected to the temperature of boiling water for a minute; at the same time four other bulbs containing the same infusions were boiled continuously for ten minutes and suspended beside their neighbours. Twelve hours subsequent to their first brief heating the eight bulbs were perfectly brilliant, and while in this condition they were again subjected to the boiling temperature for a minute. On the evening of the same day they were subjected to the boiling temperature for half a minute, and on the following morning the process was repeated. Two additional heatings of the same brief character were resorted to. The result of the whole experiment was that two days after their preparation the four bulbs which had been boiled for ten minutes were found turbid and covered with scum, while two months after their preparation the eight bulbs whose periods of boiling added together amounted only to four minutes were perfectly brilliant and free from scum.

The reason of this procedure is plain. By the first brief application of heat the germs, which are at that moment plastic, are killed; and before any of the remaining germs can develop themselves into *Bacteria* they are subjected to another brief period of heating. This again kills such germs as are sufficiently near their final development. At each subsequent period of heating the number of living germs is diminished, until finally they are completely destroyed. The infusion, if protected from external contamination, remains for ever afterwards unchanged, although, when living *Bacteria*, a sprig of hay, or even the dry dust particles of the laboratory are sown in it, the sterilized liquid shows its power both of enabling the fully developed organism to increase and multiply, and of developing the desiccated Bacterial germ into multitudinous Bacterial life.

On the same date an experiment was made with a series of pipette-bulbs, whose necks were so bent and plugged with cotton-wool that no impurity from the wool could fall into the infusion. Four bulbs were charged with an infusion of Heathfield and four with an infusion of London hay, samples of the same infusions being introduced at the same time into another series of bulbs which were plugged like their neighbours and subjected continuously to the boiling temperature for ten minutes. The eight bulbs first referred to were, on the contrary, discontinuously boiled, the sum of their periods of boiling being four minutes. The result is that while the entire series of bulbs boiled for ten minutes gave way within forty-eight hours after their preparation, seven out of the eight bulbs which had been subjected to discontinuous boiling remained permanently brilliant.

On the 3rd of February, with the view of testing the new method still further, infusions of our most refractory kinds of hay were prepared. There were five bulbs of neutralized Guildford infusion, and five of a neutralized infusion formed from a mixture of old Colchester and old Heathfield hay. Two bulbs of each infusion were at the same time charged and subjected to the boiling temperature for ten minutes. The ten bulbs first mentioned were never raised to the boiling temperature at all, the maximum to which they were exposed being some degrees below their boiling-point. The result is that while every one of the four bulbs boiled for ten minutes has become turbid and covered with scum, one only of the ten discontinuously heated bulbs has given way; nine of them remain as brilliant as at first.

It is obvious from what has gone before that two hundred and forty minutes might have been substituted for ten minutes without altering this result. Five minutes of discontinuous heating can accomplish as much as five hours continuous heating.

On the same date three bulbs charged with an acid infusion of London hay were subjected to the same discontinuous treatment. They all remain brilliant to the present hour.

On the 7th of February four of COHN'S tubes were charged with turnip-infusion, which was heated discontinuously night and morning up to a temperature of 205° Fahr. The total time during which they were exposed to this temperature was about three minutes. They were permanently sterilized, and exhibit a singular brilliancy to the present hour.

The discontinuous method of heating has also been applied with success to the closed chambers. One mode of operation is this:—An oil-bath is heated to a temperature of 300° Fahr. The charged test-tubes of the closed chamber are then plunged in the oil, which clasps the tubes to the level of the surface of the infusion. They are either raised to incipient boiling and then removed, or suffered actually to boil for thirty seconds and then removed. Another mode of heating is this: instead of being plunged into hot oil, the test-tubes are plunged for two or three minutes into boiling water, taken out, wiped dry, the actual boiling being finished by a spirit-lamp. This is a very handy method, and more under the experimenter's control than the oil-bath. When the latter is employed the infusions sometimes in great part waste themselves by leaping from their tubes; but the spirit-lamp enables us to humour the infusions by occasionally withdrawing the flame and moderating the ebullition. The lamp, of course, may be employed alone without the preliminary immersion of the tubes in hot water. Usually the process of heating is repeated at intervals of twelve hours, but in the case of very nutritive infusions in a very warm room the interval ought to be shorter. Practice must inform the experimenter on this point. The reheating must always occur before the infusions show the slightest tendency to change.

In the early days of February a closed chamber of six tubes was treated in the manner here described. Three of the tubes were charged with strong turnip and the three others with strong artichoke-infusion. After two days discontinuous heating night and morning, they were allowed to remain undisturbed in the warm room. The six tubes remain perfectly brilliant to the present hour.

On the 12th of February a closed chamber of three tubes was charged with cucumber-infusion. Heated discontinuously in the manner described, and abandoned afterwards to a warm temperature, the three tubes remained perfectly sterile.

Any process competent to sterilize very old hay can sterilize with greater ease any other infusion. The fact, therefore, that only a few days ago three closed chambers charged with our most refractory hay-infusions were sterilized by discontinuous heating proves the power of the method over infusions of all kinds.

By this method very instructive comparative experiments might be made, and the resistant power of different germs might be expressed in terms of the heatings necessary for their sterilization. I possess, for example, two test-tubes, containing the same infusion and associated with the same closed chamber, one of which has been heated five times and the other six. The former is quite turbid, while the latter is perfectly clear. In this case five heatings had left some of the more resistant germs still unkilled, which were destroyed by the sixth heating. Of two other tubes charged with a different infusion, one has been heated seven times and is now full of life; the other has been heated eight times and is perfectly barren.

With due care the method of sterilization here described is infallible, however highly infective the surrounding atmosphere may be. But here, as elsewhere in these difficult inquiries, the sagacity which comes in great part from nature, the skill which comes from training, and the care which ought to root itself in his moral constitution are all necessary to save the experimenter from error and to lead him to the truth.

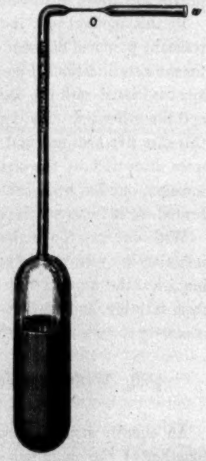
§ 23. *Mortality of Germs through defect of Oxygen produced by Exhaustion with the* SPRENGEL *Pump.*

An equally striking mode of sterilization is now to be described. The crowding together of the organisms so as to form in a multitude of cases a heavy, corrugated, fatty scum upon the surface of the infusions obviously indicated that air was a necessity of their life. In some cases the oxygen dissolved in the infusions sufficed to enable the *Bacteria* to cloud them from top to bottom; but in many cases they gathered at the top, and formed there a living layer through which no oxygen could pass to the liquid underneath, which, thus surmounted, remained as clear as water. The observation of these facts, and many others of a similar bearing, suggested inquiry into the effect which the more or less perfect withdrawal of the air from the infusions would have upon the development of life.

A few experiments with an ordinary air-pump were, in the first instance, made. The necks of a series of bulbs charged with turnip-infusion were drawn out at the middle to a tube of very small diameter. The open end of the neck being connected with the air-pump, the bulbs were exhausted. In some cases, to render the removal of the air more perfect, hydrogen was admitted into the bulb and was afterwards withdrawn by the air-pump. Before they were detached from the pump the bulbs were immersed in luke-warm water. They boiled freely, and after a minute's ebullition the narrowed necks were hermetically sealed. The bulbs were then submerged in cold water, which was gradually raised to 212° F. and kept boiling for ten minutes; they were afterwards removed and placed in a room with a temperature of about 90° Fahr.

Four bulbs were thus treated in a preliminary experiment on the 7th of March. Two of them remain crystal clear to the present hour; the two others became cloudy, but remained entirely free from scum. The cloudiness, I may add, was barely perceptible, but it was perfectly distinct to the practised eye.

Fig. 11.



By such means, however, the removal of the air must have been more or less imperfect, and I therefore resorted to the far more effective Sprengel pump. To connect them with the pump, the bulbs were thrown into the form represented in fig. 11. After the neck of the bulb had been plugged with cotton-wool it was bent at right angles above the plug, and a portion of it was drawn out to a tube of capillary diameter, represented at *o*. The end *a* was connected with the Sprengel pump, and after the exhaustion had been continued for the required interval, the neck of the bulb was sealed at *o*.

On the 14th of March three bulbs charged with the turnip-infusion, from which the air had been as far as possible removed by the ordinary air-pump, were connected with the Sprengel, which continued the exhaustion uninterruptedly for three hours. The air dissolved in the liquid freely escaped from it at first, and it continued to appear in minute bubbles long after the exhaustion had reached a considerable degree of perfection. The drawn-out necks of the bulbs being hermetically sealed, the infusion within them was maintained as before for ten minutes at the boiling temperature.

It will be remembered that when the infusion and the air above it possessed their ordinary supply of oxygen, 180 minutes' boiling failed to sterilize the turnip-infusion. Here, when the air was withdrawn from the liquid, exposure for one eighteenth of the foregoing interval sufficed to produce perfect barrenness. The infusion in the three bulbs here operated on remains to the present hour clear in body and perfectly free from scum.

On the 15th of March seven bulbs charged with infusion of turnip were treated in the manner just described, being purged of their air by three hours' action of the Sprengel pump, and boiled for ten minutes afterwards. Six out of the seven remain perfectly pellucid.

On the 16th of March the result was still further verified. Seven bulbs were then charged with turnip-infusion, exhausted first by the air-pump, and afterwards by five hours' action of the Sprengel pump. Hermetically sealed and boiled as before, six out of the seven remain as clear as distilled water.

On the 20th of March seven bulbs were charged with infusion of cucumber, and subjected to the action of the Sprengel pump for seven hours. They were afterwards treated in the manner just described. They were all completely sterilized.

On the 27th of March three bulbs were charged with cucumber-infusion and subjected to the action of the Sprengel pump for five hours. One of them was subsequently boiled for five minutes, another for one minute, while the third was left unboiled. This third tube became faintly cloudy, but the two others remain perfectly free from life.

This result invited repetition. On the 29th accordingly six bulbs of cucumber-infusion were exhausted for five hours, and afterwards sealed and boiled for a single minute. Five of the six bulbs remained permanently clear; one became cloudy.

On the 30th of March six bulbs containing turnip-infusion were exhausted for five hours and boiled afterwards for a minute. Five remain perfectly clear, one has become muddy.

On the 6th of April five bulbs of beef-infusion were subjected for three hours to the action of the Sprengel pump and boiled for a minute afterwards. They all remain brilliant.

On the 7th of April five bulbs of mutton-infusion were treated like the beef-bulbs, being exhausted for three hours and boiled for a minute. All remain clear. This experiment was repeated and confirmed on the 20th of April.

On the 14th of April three bulbs of pork-infusion were exhausted for four hours and boiled for a minute. They all remain pellucid.

On the 17th of April four bulbs of accurately neutralized urine were exhausted for five hours and boiled for a minute. Three of them remain bright; one has become cloudy.

This does not exhaust the list of instances. Many other infusions have been sterilized by this method since the 17th of April.

It is perfectly certain that in most, if not all, of these cases 200 minutes' boiling would have proved insufficient to sterilize the infusion if it had been supplied with air.

Here the question naturally arises:—What would happen if the bulbs were exhausted and left unboiled? Probably with sufficiently perfect exhaustion all infusions would be sterilized. But in the trials I have thus far made some of the unboiled infusions have become cloudy, while others have remained clear. Thus three bulbs of mutton-infusion exhausted for four hours, two bulbs of beef-infusion exhausted for three hours, four bulbs of pork-infusion exhausted for four hours and left unboiled remain as transparent and as free from life as their boiled companions. Various other instances of sterilization without boiling might be cited. On the other hand, three bulbs of neutralized urine, exhausted for five hours and left unboiled, became cloudy. A case of cucumber-infusion which behaved similarly has been cited above. It is difficult, if not impossible, to remove from the infusion and the space above it the last traces of air; and when backed by a highly neutritive liquid an infinitesimal residue of oxygen can develop a sensible amount of life. I may add that I have tested the exhaustion of some of the cloudy bulbs, and have found it in every case defective.

The foregoing instances sufficiently illustrate the dependence of the organisms here under review upon the supply of oxygen*. I think it probable that the principle thus indicated is capable of useful and extensive practical application.

§ 24. *Mortality of Germs through defect of Oxygen consequent on boiling the Infusion.*

Long prior to these experiments with the Sprengel pump, the influence of atmospheric oxygen on the life of these organisms had been brought home to me. It revealed itself in a striking manner in experiments with infusions purged of air by boiling, the vessels containing them being carefully sealed during ebullition. At a time when the atmosphere of our laboratory was so laden with infection that no escape for animal or vegetable liquids introduced in the usual way into closed chambers was possible, it was perfectly easy to keep the same infusions pellucid for an indefinite time in vessels purged of air by boiling and properly sealed. I will give a few out of the multitude of examples that might be cited in proof of this statement.

On the 2nd of October fourteen of our ordinary retort-flasks (fig. 4) were charged with a neutralized infusion of hay. They were boiled for three minutes in an oil-bath, and hermetically sealed whilst boiling. Thirteen out of the fourteen tubes remained perfectly barren, retaining for months their pristine colour and transparency.

On the 18th of November six retort-flasks were filled with turnip-, five with cucumber-, five with beetroot-, and four with parsnep-infusion. The six turnip-flasks remained permanently pellucid, yielding a clear water-hammer ring. The five beetroot-flasks remained also permanently barren, all yielding the water-hammer sound. Of the parsnep-flasks, two became turbid, but two remained clear. Of the cucumber-flasks, three became cloudy, while three remained permanently clear. Neither in the case of the parsnep nor in that of the cucumber could the water-hammer sound be obtained from the cloudy flasks, and when their sealed ends were broken under water, the vacuum was found defective. In the clear tubes, on the contrary, it was found practically perfect.

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* In search of this gas they sometimes rise into the liquid film which covers the interior of the bulb to the height of an inch and more above the surface of the liquid, forming within the bulb a gauzy scum which appears as if lifted by capillary attraction. Their mode of nutrition must therefore be very different from that of *Torula*-cells, as revealed by the excellent researches of PASTEUR.

Again, on the 20th of November, seventeen retort-flasks were charged with infusions of turnip, cucumber, and parsnep. They were boiled for three minutes in an oil-bath, and carefully sealed while boiling. The six turnip-flasks remained permanently clear, maintaining for months their sharp water-hammer sound. Of the five parsnep-flasks, one became turbid and four remained permanently clear. These latter only yielded the water-hammer sound. The turbid flask, on the contrary, when shaken yielded no such sound, and when its sealed end was broken under water, its vacuum proved defective. Of the six cucumber-flasks, two became turbid, the remaining four being perfectly clear. On breaking their sealed ends under water, one third of one of the turbid flasks and one fourth of the other remained unfilled by the liquid.

On the 6th of December eighteen retort-flasks were charged with cucumber-infusion. They were boiled for the usual time, that is three minutes, extreme care being taken to seal them during the issue of the steam. The water-hammer sound in all these flasks was particularly sharp and clear. Exposed to a temperature of 90° Fahr. for many weeks, seventeen of them remained perfectly pellucid; while the same infusion in a sealed bulb with filtered air above it and dissolved in it swarmed with life after boiling for sixty times the interval here found effectual.

On the day subsequent to its preparation, one of these well-exhausted flasks was unexpectedly found turbid and covered with scum. But on examining the sealed end it was found snipped off. The laboratory air had thus entered the flask and given birth to the observed organisms. Failures of this sort have a demonstrative force greater even than successes; they render so obviously plain the external source of the contamination.

It is worth saying here that the observation just recorded was of frequent occurrence. The fineness of the sealed points of our retort-flasks renders them very liable to be snipped off if they are not handled with care. After preparation they are usually suspended on a wire or on a wooden support; and in frequent instances, after such suspension, I have found a flask differentiating itself by thick turbidity from a number of perfectly pellucid neighbours, the yielding of the flask being immediately traced to the fracture of its sealed end.

With the view of showing how readily, unless extreme care is taken, contamination may enter hermetically sealed vessels, the following experiment was made on the 6th of December. Four retort-flasks were charged with the cucumber-infusion, boiled for the usual time and sealed, not during the outrush of the steam, but a moment after ebullition had ceased. On the 9th of December three of these four flasks were faintly but distinctly turbid. The reason is obvious. On the cessation of the ebullition, a momentary condensation of the steam above the infusion caused an indraught—slight, no doubt, but still sufficient to contaminate or vivify the infusion.

The source of the contagium was also indicated by the following experiments. A large number of retort-flasks, embracing infusions of snipe, wild duck, partridge, hare, rabbit, mutton, turbot, salmon, whiting, mullet, turnip, and hay, had remained over from my stock of 1875. After a year's exposure to the temperature of our warm room not one of these flasks showed the slightest trace of turbidity or life. On the 7th of December the sealed ends of forty of them were snipped off in the laboratory. Five days afterwards twenty-seven of them were found swarming with organisms—a considerably higher percentage than that obtained by the same process in the same laboratory a year previously.

It is needless to dwell with any emphasis on the obvious inference from all this, namely, that the contagium is external to the infusions (that it is, something in the air), and that at different times we have different amounts of aerial interspace free from the floating contagium.

§ 25. *Critical Review of the last two Sections.*

It has been my desire and aim throughout this inquiry to free it as much as possible from uncertainty and doubt. I have tried to render the facts safe by laborious repetition, and to render the interpretations of those facts secure by close and constant criticism. Thus, in reference to our present subject, I had to put to myself very definitely the question whether the permanent clearness of an infusion exposed to a very moderate amount of heat, after having been freed from air by boiling or by the Sprengel pump, was really due to the destruction of the germs in the infusion. Even in a highly infective atmosphere from three to five minutes' boiling in an oil-bath suffices to sterilize our retort-flasks, while it is perfectly certain that exposure to the boiling temperature for fifty times this interval fails to kill the germs of an infusion containing a good supply of atmospheric air. A similar remark applies to our experiments with the Sprengel pump. I asked myself whether in these cases the life of the germs was not suspended merely, instead of being destroyed. It was quite conceivable that germs endowed with vital power, ready to act under proper conditions, might still exist in our hermetically sealed flasks, although the entire absence of oxygen rendered their further development impossible.

That something more than a mere temporary hindrance to development is here involved was, however, proved by many of the experiments just recorded. These experiments showed that after hermetically sealed flasks had remained pellucid, not only for days but for weeks and months, and in some cases for more than a year, when their sealed ends were broken off, even in ordinary air, they by no means invariably showed signs of life afterwards. Many of them remained permanently barren while copiously supplied with oxygen.

Special experiments were, however, made to illustrate this point. First of all, as I have already recounted, hermetically sealed retort-flasks were opened in one of our lower store-rooms, and though supplied with oxygen from this source they showed subsequently no signs of life. A considerable number of retort-flasks had also their sealed ends broken off in the midst of a spirit-lamp flame. It is known that gunpowder can be dropped through such a flame without ignition; and in a few rare instances the infusions which had their first supply of air thus passed through a flame showed subsequent signs of life. In such cases the germinal matter had been sucked so rapidly through the flame that it escaped destruction; but in the vast majority of instances the sterilized infusions remained sterile.

Mechanical arrangements were also made for the breaking off of the sealed ends in a receiver filled with filtered air. But it is by no means easy to perfectly cleanse from infectious matter the instruments used in such experiments, though with sufficient practice this might certainly be done. The consequence was that some of the flasks opened in filtered air yielded subsequently. The following is an illustrative case:—On the 3rd of January ten flasks were charged with cucumber-, turnip-, artichoke-, and melon-infusions. They were boiled for the usual time, sealed during ebullition, and exposed afterwards to a warm temperature. Their sealed ends were then broken off by a mechanical contrivance placed in a receiver containing filtered air. The two artichoke-flasks remained permanently barren afterwards. Of the two melon-flasks, one remained barren and the other developed life. Of the two cucumber-flasks, one became turbid, the other remained clear. Of the four turnip-flasks, two became turbid and two remained clear. Out of the ten flasks, therefore, all freely connected with the external air, six remained permanently barren. By further practice barrenness in almost every instance has been secured. The conclusion, I think, is obvious. It is not the heat alone that destroys these germs, for fifty times the amount of heat will not accomplish this when oxygen in due quantity is present: the heat must be aided by the withdrawal of the oxygen.

§ 26. *Mortality of Germs through excess of Oxygen.*

The foregoing remarks lead naturally to a brief reference to the important experiments of M. PAUL BERT[*] on the toxic influence of compressed oxygen. From the imperfect account of these experiments which first reached me, I inferred that the germs of putrefaction had been destroyed by mere mechanical pressure, and more than a year and a half ago I placed turnip-infusions in strong iron bottles, and subjected them for several days to an air-pressure of twenty-three atmospheres. When taken from the bottles the infusions were found one and all swarming with life. Last October I made a series of similar experiments with infusions of hay and turnip, subjecting them for several days to an air-pressure of twenty-seven atmospheres. When taken from their iron bottles the infusions were found one and all teeming with *Bacteria*.

I then resorted to pure oxygen, and found the same to be true of my infusions that M. PAUL BERT had found true of his flesh, moist bread, boiled starch, strawberries, cherries, wine, and urine. Pressures varying from twenty-seven atmospheres to ten atmospheres of oxygen were employed. In all cases, however long the pressure was continued, or however favourable to putrefaction the surrounding temperature might be, the infusions (which embraced those of beef, mutton, and turnip) were found, when taken from the bottles, as clear as crystal and entirely free from life. It required, indeed, long subsequent exposure to the common air to infect infusions which had been thus surcharged with oxygen. Other bottles containing the same infusions were simultaneously subjected to a like pressure, not of oxygen, however, but of atmospheric air. When removed from the bottles they were one and all found in a state of putrefaction and swarming with life.

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[*] Comptes Rendus, vol. lxxx. p. 1579.

Thus when oxygen is wholly withdrawn from organic infusions, the life with which we are here concerned ceases. When, on the other hand, the gas is in considerable excess, it becomes a deadly poison to organisms which, in moderate quantities, it sustains. As in the case of temperature, so in regard to the supply of oxygen, there is a median zone favourable to the play of vitality, beyond which, on both sides, life cannot exist.

The present memoir virtually ends here; but I will append a few brief sections which, though incomplete, are not without instruction.

§ 27. *Experiments on neutralized Urine.*

I have already communicated to the Royal Society the result of some experiments made with this liquid[*], in which the potash employed for neutralization was subjected to a temperature of 220° Fahr. The alkali was contained in tubes drawn out finely at the end, which were introduced into the flasks containing the urine, and broken by shaking after the acid urine had been completely sterilized by heat. The cases were exceedingly rare in which life showed itself in the urine thus neutralized. The preponderance of sterilized flasks over unsterilized ones was enormous.

In the experiments now to be glanced at, neither the urine nor the potash was raised to a temperature higher than 212°. Wishing to ascertain how the refractory germs of our laboratory would fare in neutralized urine, on the 16th of February I had five pipette-bulbs charged with the liquid. It was neutralized by caustic potash, which on boiling produced copious precipitation. It was afterwards filtered and rendered very transparent. The bulbs had been well cleansed, filled with one third of an atmosphere of filtered air, hermetically sealed, and exposed afterwards to the heat of a Bunsen flame. They were charged with the urine by breaking off their finely drawn out points in the body of the liquid. They were then again sealed, and subjected to the boiling temperature for ten minutes.

Not one of these bulbs remained sterile. Two days subsequent to their preparation they were all swarming with organisms.

Three other bulbs were on the same occasion charged with precisely the same infusion, only instead of being associated with air they were well purged of air by five minutes' boiling in an oil-bath. While the steam was issuing they were hermetically sealed.

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[*] Proceedings, vol. xxv. p. 457.

Not one of these bulbs has proved fruitful. They are all as brilliant and as free from life as they were after they had passed through the filter.

The difference here indicated is worthy of notice. In the one case five minutes' action completely sterilizes; in the other ten minutes' action fails to do so. This latter interval indeed might be multiplied twentyfold and still prove ineffectual. In the one case the process of boiling purged the liquid of its air; in the other case the air was retained within and above the liquid. The case therefore connects itself with our former illustrations.

On the 21st of February six bulbs were charged with fresh urine carefully neutralized and boiled for five minutes in an oil-bath. Of the six flasks, four remain perfectly clear and brilliant, one is slightly cloudy, and one turbid.

The urine here referred to was neutralized in our own laboratory; but as the importance of accurate neutralization has been much insisted on, I wished to check myself. At my request, therefore, Dr. Debus was good enough to send me from Greenwich a quantity of urine carefully neutralized by him. On the 1st of March seven retort-flasks were charged with the neutralized liquid. These were boiled for five minutes in an oil-bath and sealed during ebullition. Three of these flasks have become turbid, but four remain perfectly clear.

On the 5th of March Dr. Williamson was good enough to send me a supply of neutralized urine collected in a public urinal in University College. The colour was very deep, the odour was very bad, and the precipitation on boiling very copious. Fourteen retort-flasks were charged with this liquid on the 6th of March. Seven of them have gone bad, but seven of them remain clear.

On the 10th of March Dr. Frankland was good enough to send me a supply of urine neutralized by himself. It was introduced into four retort-flasks, which, like the others, were boiled for five minutes in hot oil and sealed during ebullition. None of these flasks have shown the slightest sign of yielding. The liquid within all of them is as brilliant as it was when first introduced.

In every case here mentioned the liquid, after boiling, was exposed for several days to a temperature of 50° C.

The conflict described in the foregoing pages and the search for principles to reconcile the results occupied me too long to permit of my doing more than break ground on the subject of urine. I entertain, however, a strong opinion that by a little practice with this liquid, its sterilization by five minutes' boiling might be rendered certain in every case. As the experiments stand, they sufficiently negative the conclusions of Dr. Bastian, who has brought forward the deportment of neutralized urine as a specially convincing illustration of spontaneous generation. Nor are they in accordance with the statement of M. Pasteur, that in neutralized urine, subjected only to the ordinary boiling temperature, organisms in the majority of cases ("le plus souvent") appear.

§ 28. *Hermetically sealed flasks exposed to the sun of the Alps.*

A remark of Dr. Bastian's, wherein he refers to the power of the actinic rays of the sun to promote spontaneous generation*, caused me to take with me last summer to Switzerland a number of flasks hermetically sealed with special care and charged with infusions of various kinds. Eighty of them were carefully packed in sawdust in London; but on my arrival at the Bel Alp, which stands at an elevation of some 7000 feet above the sea, I found only forty-five of them unbroken. They were thus distributed:—

Beef	12 flasks.
Mackerel	12 „
Turnip	12 „
Fowl	9 „

For ten days of the splendid summer with which we were favoured during a portion of last July, these flasks were exposed daily to the sunlight upon the roof of the Bel Alp hotel. The sky during many of these days was of a deep and cloudless blue; and certainly in London the actinic rays never approached the power of those here brought to bear upon the infusions. The temperature for many hours of each day was about 120° Fahr. Every evening, when the thermometer had fallen to about 70°, the flasks were removed and suspended above the kitchen-range of the hotel, the temperature generally varying throughout the night from 70° to 80° Fahr. Such variations of temperature, it may be remarked, are deemed by Dr. BASTIAN favourable to spontaneous generation.

After the sunny weather had disappeared, the flasks were allowed to remain for three weeks suspended in the kitchen, with occasional exposures to the sun; the average temperature of the kitchen where the flasks were hung was about 90° F. The result of the observations was that not one of these forty-five flasks yielded the slightest evidence of spontaneous generation. From first to last they all continued as limpid as distilled water.

The sealed ends of these flasks were afterwards snipped off under various circumstances, some on the Sparrenhorn, some on the glacier, some in the Massa Gorge, some amid the hair of my own head, and some in the rooms of the hotel. Many of them, moreover, were infected with water of various kinds—spring-water, lake-water, and glacier-water. It is not my object to give a detailed account of these experiments, but simply to say that it was not lack of nutritive power on the part of the infusions which prevented the appearance of organisms in the first instance; for when brought into contact with infectious matter every one of the flasks showed its power of sustaining and multiplying life.

§ 29. *Remarks on Hermetic sealing.*

A few brief remarks on this subject may, I think, be fitly interpolated here. Hermetic sealing during ebullition is an operation requiring some apprenticeship to perform it aright. The neck of the flask ought to be so narrow that the pressure of the steam within shall be always sensibly greater than that of the atmosphere without. This condition would be readily fulfilled if the liberation of the steam were absolutely uniform, and not by fits and starts. But it never is uniform, and if the channel through which the steam issues be wide, it is scarcely possible to avoid regurgitation. Sometimes the pressure within is above that of the atmosphere, and steam freely issues; but at the next moment, through liquid adhesion to the flask and partial condensation above, the internal pressure may be below that of the atmosphere and permit air to enter. This alternate triumph of the inner and the outer pressure may be rendered plainly evident by the motions of the water condensed in the neck of the flask. The liquid acts as an index which moves to and fro, sometimes forward, sometimes backward, as the pressure varies. It is quite evident that contamination may be, and it is quite certain that contamination has been, thus introduced into flasks reputed to be free from air.

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* Nature, vol. iii. p. 247.

Even with considerable care and highly disciplined manipulatory skill success is not invariable. Ten per cent. is not at all a large allowance to set down as defective in ordinary hermetically sealed flasks. The recent opening of about two hundred flasks employed in my earlier experiments under water and under caustic-potash solution furnishes the basis of this conclusion. Even in a comparatively pure atmosphere success does not in every instance attend the experimenter. At Kew, for example, on the 8th of January, thirteen retort-flasks were charged with infusions of cucumber, melon, beef, and sole. Twelve out of the thirteen remained perfectly limpid, but one of them (a cucumber-flask) became distinctly cloudy, and this one alone refused, when tested, to yield the water-hammer sound.

§ 30. *Experiments with Turnip-cheese infusions.*

I am unwilling to omit all reference to experiments which have cost considerable labour, and which, though they have not been repeated and controlled to the extent that I could wish, contribute nevertheless to our knowledge of the present question. This unwillingness causes me to introduce here, in the briefest manner possible, a reference to a series of experiments made with turnip-cheese infusions, so frequently cited by Dr. BASTIAN as offering a conspicuous proof of the doctrine of spontaneous generation.

The experiments to which I refer were made in part with closed chambers and in part with hermetically sealed retort-flasks. The specific gravity of the infusions varied from 1008 to 1012. The cheeses employed were Cheshire, Cheddar, Gloucester, Dutch cheese, American cheese, Roquefort, and Parmesan, the quantity varying from half a grain to two grains for every ounce of the infusion. The cheese being first well triturated in a mortar, so as to render its particles very minute, was intimately mixed with the infusion, which was then boiled for a few minutes and passed through a filter. The filtered liquid was then introduced into its closed chamber, and boiled there for five minutes.

Sixteen such chambers were employed, one of them containing twelve test-tubes, each of the others only three. There were therefore fifty-seven test-tubes in all. The result of the experiment was, that out of the fifty-seven tubes twenty-seven became turbid in a few days, while thirty remained for months without sensible alteration*.

A considerable number of retort-flasks were charged at the same time with the same infusions, and boiled for five minutes in an oil-bath. The great majority of these flasks remained perfectly intact.

Here, then, as elsewhere, the ground on which the doctrine of spontaneous generation has sought to plant itself slips from under it; for assuredly the scientific mind will attribute to other causes than to it the production of organisms in the minority of cases above referred to.

One likely cause may here be signalized and illustrated. The experiments of SPALLANZANI on the action of heat upon seeds are well known, and they have been frequently cited by Dr. BASTIAN in support of his favourite thesis that the briefest exposure to the temperature of 212° Fahr. suffices to destroy all living matter. I have repeated many of SPALLANZANI'S experiments, and will here briefly refer to one series which bear upon the present point. Peas, kidney-beans, cress- and mustard-seed were tied up in small calico bags, and boiled for intervals varying from half a minute to five minutes. They were then carefully sown in flower-pots filled with well-prepared earth, and placed in a shed kept at a temperature of 70° Fahr. An unboiled sample of every seed was sown at the same time beside the boiled ones. The unboiled seeds sprouted vigorously. Thirty seconds' exposure to the boiling temperature deprived both the peas and the beans of their power of germination. A few of the cress-seeds exposed for this interval sprouted, but the majority were killed, and all were killed by a minute's boiling. On the other hand, a very large proportion of the mustard-seeds boiled for thirty seconds germinated. The time of exposure in the case of this seed was doubled, trebled, and quadrupled, leaving still a residue of life. The fertile mustard-seeds gradually diminished in number as the time of boiling increased, but even after two minutes' boiling many of them germinated.

And now comes a fact which I deem of some importance as regards the present inquiry. When the calico bag was abandoned, and the mustard-seeds were placed loosely in water, so as to ensure not only the free communication to them of its temperature, but free diffusion between the soluble portions of the seeds and the surrounding liquid, not one of them escaped the ordeal of thirty seconds' boiling. In the first series of experiments, the bag which held the seeds together not only exercised a protecting influence itself, but it enabled the outside seeds to act as shields to the inside ones. Assuredly in a far higher degree will cheese shield germs contained within it. Unlike fruit and meat it is highly impervious to water. It thus wards off the liquid on which the softening and swelling of the germ depend, so that within such a substance the life of a germ might be indefinitely prolonged.

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* These chambers were prepared and their tubes charged prior to the introduction of hay into our laboratory last autumn, otherwise the immunity of a single one of them could not have been secured. The chambers employed had stood over from my last investigation, and no pains had been taken to render them air-tight.

VI. *On the Absorption-Spectra of Bromine and of Iodine Monochloride.*

By H. E. Roscoe, *F.R.S., and* T. E. Thorpe, *F.R.S.*

Received March 16,—Read May 4, 1876.

[Plate 4.]

The element bromine and the compound iodine monochloride possess nearly the same molecular weight. The colours of their vapours appear almost identical, and a rapid glance at the complicated absorption-spectra afforded by the two gases fails to detect any difference between them. It becomes, therefore, a matter of importance to ascertain whether the molecules of the two bodies, when gaseous, vibrate identically or similarly. We have accordingly undertaken a series of exact measurements of the absorption-spectra of these two gases, the results of which we have now the honour to communicate to the Royal Society.

The two spectra were compared simultaneously by means of one of **Kirchhoff's** model spectroscopes, 4 flint-glass prisms of 60° and 45° and a magnifying-power of 40 being employed. The position of the lines and of the well-defined edges of the bands in both spectra were read off by reflexion upon a fixed arbitrary scale; the positions of 27 air-lines lying between the extremes of the absorption-spectra were then determined upon the same scale, both the scale and prisms remaining untouched during the different series of observations. In order to determine the wave-lengths of each of the absorption-bands in the spectra of bromine and iodine monochloride, the wave-length of each of the 27 air-lines was ascertained by reference to the measurements of **Thalén**; in three cases, marked H, in the accompanying Table the numbers given by **Huggins** have been used.

Colour of ray.	Micrometer-scale reading.	Wave-length.	Colour of ray.	Micrometer-scale reading.	Wave-length.
Red	42·48	6602	Yellow ...	53·06	5666
	42·96	6562		54·81	5549
	43·61	6480		55·00	5541
Orange ...	46·81	6171		55·09	5534
	49·30	5949		55·17	5530
	49·47	5942		55·25	5524 H.
	49·54	5932		55·61	5495
	49·68	5929		55·86	5479
Yellow ...	51·68	5767		56·18	5462
	51·96	5745		56·29	5453
	52·48	5711		58·10	5350 H.
	52·83	5686	Green......	58·27	5339
	52·88	5680 H.		58·59	5320
	53·00	5675			

In order to reduce the micrometer-scale readings of the absorption-spectra to wave-lengths, a graphical method was adopted. In the first place, the wave-lengths of the 27 air-lines having been found as described, these numbers were taken as ordinates and plotted on curve-paper. the scale-readings of the air-lines being taken as abscissæ. The points thus obtained fell naturally into six groups or series, and a mean point for each group was then found by accurate measurement, and the curve drawn through the six mean points. In order to obtain the wave-lengths of the well-defined edge of the different bands of the absorption-spectra, it was merely necessary to ascertain from the curve the value of the ordinate corresponding to each scale-reading as abscissa. The positions of the six mean points, each corresponding to a group of air-lines needed for the construction of the curve, were ascertained by means of the arrangement seen in Plate 4. fig. 1. The wooden rod (A A) has a steel point (C C) fixed at one end, and a graduated paper scale at the other. At the end carrying the steel point is a strip of wood (T T P) placed at right angles to the rod, which it serves to support, so that the axis (C C) of the steel point is vertical, whilst the scale (S S) lies in a horizontal position, the end P sliding on the table when the rod is rotated round the point C C as centre. The curve-paper is now adjusted on the table, so that some one division on the scale describes a circle through one particular group of spots and also through two other groups, one on each side, which circle, if drawn, would pass not far from the mean points of the three groups.

In order now to find the exact position of the mean point of the middle group through which the curve is to pass, the distance from each point to the steel centre (C C) of the circle is first accurately read off on the divided scale. A point is then chosen and plotted near the middle of the group, as a mean of the several distances, such that the sum of the shortest distances of the several points of any one group to the curve drawn through this mean point is zero.

This process is repeated for each of the four groups in the middle, the centre of curvature for an end group being taken to be the same as that for the second group from that end. In one case an isolated point occurs, and this is taken as one group and the curve drawn through it.

The curve consists of arcs of circles which pass through the six mean points, with radii varying from 64¼ inches in length at the upper end to 61½ inches at the lower end.

Absorption-Spectrum of Bromine.

This absorption-spectrum was first examined in 1833 by W. H. Miller, but no accurate map has hitherto been published.

The bromine employed in our experiments was a portion of the sample used by one of us in the determination of the rate of thermal expansion of this liquid. It was obtained by distilling a large quantity of commercial bromine, after dehydration with oil of vitriol, and collecting separately the fraction boiling at about 60°, which amounted to about two thirds of the whole liquid operated upon. The sample was found to be free from iodine. In order to remove any chlorine which might be present, it was digested with powdered potassium bromide for several weeks, again distilled, and treated with phosphorus pentoxide. The pure bromine boiled constantly between 59°·6 and 59°·7 (corrected). Bar. (corrected and reduced) 765·2 millims.

The spectrum was obtained by allowing the rays from a lime-light to pass, before falling on the slit of the spectroscope, through bromine vapour contained in a long and strong narrow glass tube heated by means of a flame. The spectrum thus obtained is a channelled one, and consists of a large number of bands, 66 of which have been measured. Each band, like those of iodine[*], is made up of a close association of fine lines, the whole stretching from wave-length 6801·5 in the red to wave-length 5244·1 in the green. Beyond these extremes the general absorption becomes too powerful to permit further bands to be seen. Each of these bands has a well-defined edge towards the blue end, and shades off gradually towards the less refrangible end of the spectrum, as shown in the drawing. The bands are by no means regularly dispersed throughout this length, but are more numerous, as well as more intense, in the yellow and green portions of the spectrum than in the red. The bands in the green form the best defined portion of the bromine spectrum, and the bands 5483·8 and 5460·1, although rather fainter than some others, are especially characteristic. The spectrum of the vapour of bromine at the ordinary temperature and that obtained when the vapour is heated differ from each other, inasmuch as the lines in the green beyond 5433·2 and in the red beyond 6101·4 are invisible in the cold, though visible in the heated vapour. A few lines, such as 5634·8 and 5580·6, in the green appear in the spectrum of the heated vapour, but not in that of the cold vapour; but, as a whole, the bands seen in the cold vapour are also seen in the heated vapour, and no change whatever in position is noticed. The least refrangible bands beyond 6526·9 are ill-defined; they appear directly upon heating, but fade quickly away as the vapour cools. The letter *f* placed after the wave-length denotes that the line is faint; *v.f* means very faint; and *s* denotes that the line is dark and well-defined.

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[*] THALÉN, 'Le Spectre d'absorption de la vapeur d'Iode,' Upsal, 1869.

I. Absorption-Spectrum of Bromine.

Colour of rays.	Wave-lengths.	Remarks.
Red	6801·3	
	6777·2	
	6723·9	
	6649·1	
	6581·3	
	6526·9	
Orange ...	6468·9	...... *s.*
	6455·4	...... *f.*
	6413·0	...... *s.*
	6401·0	...... *f.*
	6372·6	...... *f.*
	6350·5	...... *s.*
	6336 7	...... *f.*
	6312·1	...... *f.*
	6292·8	...... *s.*
	6275·4	...... *f.*
	6263·9	...... *f.*
	6240·2	...... *s.*
	6223·3	...... *f.*
	6190·9	
	6169·7	
	6144·1	
	6119·0	
	6101·4	
	6072·2	...... *s.*
	6053·2	...... *f.*
	6027·3	
	6006·1	...... *f.*
	5987·5	...... *s.*
	5956·5	...... *s.*
	5945·1	...... *f.*
	5913·9	...... *s.*
	5905·9	...... *f.*
	5875·5	
	5870·7	...... *v.f.*
Yellow ...	5835·3	
	5797·7	
	5762·7	
	5727·5	
	5694·4	
	5660·4	
	5634·8	
	5624·4	
	5592·0	...... *s.*
	5580·6	...... *f.*
	5560·7	...... *s.*
	5556·8	...... *f.*
	5534·1	
	5510·3	
	5501·3	
	5483·8	
	5476·8	
	5460·1	
	5439·9	
	5418·2	
	5403·2	
	5380·3	
	5365·8	
	5347·5	
Green......	5337·4	
	5306·8	
	5298·7	
	5292·2	
	5274·5	
	5258·8	
	5244·1	

Absorption-Spectrum of Iodine Monochloride.

This compound was prepared by heating a mixture of iodine and finely powdered potassium chlorate. After repeated distillation from potassium chlorate it boiled constantly between 99°·7 and 100°·7. Corrected and reduced barometer 744·3 millims.

The spectrum of iodine monochloride is likewise a channelled one, generally resembling that of bromine, but differing essentially from it as well in the position of the bands as in their distribution. For the purpose of mapping its absorption-spectrum the chloride was placed in a cylindrical glass tube, 14 centims. in length, having flattened ends, the rays from the lime-light passing through the length of the tube, which was then heated with a lamp.

The spectra of the moderately heated and strongly heated vapour exhibit differences similar to those seen in the case of bromine. Thus at the higher temperature a great increase in the number of bands in the green is observed, none of the bands from 5679·5 to the end (5276·1) being visible at the lower temperature. No change in the position of the bands or lines common to the vapour at both temperatures is, however, observed on heating or cooling, so that in other respects, and as regards by far the majority of the lines or bands, no alteration is produced by heat.

II. Absorption-Spectrum of Iodine Monochloride.

Colour.	Wave-length.	Remarks.	Colour.	Wave-length.	Remarks.
Red	6475·1		Yellow ...	5788·8	...... *s.*
Orange ...	6442·9			5782·0	...... *f.*
	6421·3			5751·0	
	6383·7			5744·4	...... *v.f.*
	6372·6			5719·6	...... *s.*
	6324·9			5713·0	...... *f.*
	6318·0			5685·8	
	6266·8			5679·5	
	6216·9			5658·3	
	6181·5			5650·0	
	6167·9			5632·1	
	6155·0			5628·6	
	6122·6			5618·4	
	6112·8			5600·7	
	6079·2			5590·0	
	6071·3			5572·0	
	6040·9			5561·3	
	6033·2			5552·9	
	6021·3	...... *f.*		5535·4	
	6005·2	...... *s.*		5523·6	
	5995·9	...... *f.*		5508·4	
	5974·1	...... *f.*		5501·3	
	5957·3	...... *s.*		5482·5	
	5944·3	...... *f.*		5459·5	
	5918·7			5435·1	
	5905·1			5412·1	
	5886·7			5394·3	
	5877·8			5368·1	
	5861·4			5349·8	
Yellow ...	5852·3			5330·0	
	5843·7		Green......	5315·5	
	5820·5	...... *s.*		5295·0	
	5815·9	...... *f.*		5276·1	

A careful comparison of the absorption-spectra of bromine and iodine monochloride shows that although a large number of bands are nearly coincident, the spectra, as a whole, are not identical either when the vapours are examined at low or at high temperatures, or when the length of the columns of absorbing gas is varied. Out of the 66 lines mapped in each of the two spectra only two, viz. 6475·1 and 6372·6, appear to be truly coincident. The differences between the two spectra are seen, first, in the red portion, inasmuch as six bands are observed in the bromine beyond the last, 6475·1, in the iodine monochloride; and, secondly, in the green portions, where the chloride spectrum exhibits a larger number of closely set or double lines than are seen in the corresponding portions of the spectrum of bromine.

The curve and the map, on half the scale of ANGSTRÖM'S "spectre normal," have been very carefully drawn by Mr. WILLIAM DODGSON, to whom we beg to tender our thanks.

[NOTE.—Since the above paper was read, Professor STOKES has suggested to us that the wave-lengths might have been more easily determined by means of a curve in which the coordinates were the deviation, less a constant, and the squared reciprocals of the wave-length, less a constant, a curve which would come out almost a straight line. We communicated this suggestion to Mr. A. M. WORTHINGTON, who has worked it out, and who endorses Prof. STOKES'S recommendation.]

VII. *On the Organization of the Fossil Plants of the Coal-measures.*—Part VIII. *Ferns* (continued) *and Gymnospermous Stems and Seeds. By* W. C. Williamson, *F.R.S., Professor of Natural History in the Owens College, Manchester.*

Received May 2,—Read May 18, 1876.

[Plates 5–16.]

In one of his valuable Memoirs on the Fossil Plants of the Carboniferous deposits of France* M. Renault has described the stems of two fossil Ferns, to both of which examples characteristic petioles are attached. In these stems the chief vascular or pseudo-vascular elements are not scattered over the transverse section of the stem in detached bundles, as is usual amongst living Ferns, but they are gathered together so as to form an axial cylinder, enclosing an irregular, central, radiating medulla composed of parenchymatous tissue. In the sixth of this series of memoirs I figured (plate 58. fig. 51) a transverse section of a similar vascular axis, but without either petioles or even any investing cortical layer attached to it. In one of M. Renault's examples the petioles associated with the stem were identical with those to which Corda gave the name of *Zygopteris*. In the second species, to which M. Renault assigned Corda's generic name of *Anachoropteris*, the transverse section of the petiolar vascular bundle resembles that of *Osmunda* and *Todea*, as M. Renault has correctly indicated (*loc. cit.* p. 176). He also points out the difference existing between the closed vascular cylinders of the stems of these two Ferns and the corresponding bundles of most living types, but thinks that something similar to them may be found amongst the *Ophioglossums*.

I am indebted to my indefatigable auxiliary, Mr. J. Butterworth, for an interesting stem of the same type which he obtained from the rich reservoir of fossil plants near Oldham. The specimen was about 1½ inch in length, whilst its somewhat oval, transverse section had a maximum diameter of about an inch. The external surface was strongly marked by an irregular series of transverse ridges and furrows, represented in fig. 1, and which may some day contribute to the identification of the fronds with which this stem should be associated. There are about twenty-four of these ridges, with corresponding intermediate furrows, to each vertical inch of the stem. On making a transverse section of the specimen I found a central vascular cylinder closely resembling that seen in M. Renault's specimen of *Zygopteris Brongniarti* (*l. c.* pl. 3. fig. 1, *a*), along with two or three secondary ones, of varying dimensions, dispersed over the area of the section. Utterly unable to interpret the relations of these several structures, I adopted the plan which proved so successful in the case of *Heterangium Grievii* (Phil. Trans. 1873, pl. xxx. figs. 37–44), and had about an inch of the specimen cut up into the series of closely consecutive, transverse sections represented in figs. 2–12. By this process a partial interpretation of these sections, at least, has become sufficiently easy.

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* "Étude de quelques végétaux silicifiés des environs d'Autun," par M. Renault, Annales des Sciences Naturelles, 5ᵉ série, Bot., t. xii. (Cahier no. 3).

Vascular cylinder.—The central vascular cylinder (figs. 2–12, *b*) consists of from five to seven clusters of vessels, which clusters are intimately blended together at their peripheral borders. This arrangement is more clearly shown on the enlarged scale of 18½ diameters in fig. 13.

Medulla.—The medullary cells constitute a central mass (*a*), which sends off several irregular, narrow, diverging radii (*a'*, *a'*). These diverging portions, which occasionally dichotomize, as is the case with the one descending to the lower portion of fig. 13, partially separate the clusters of vessels from each other. The cells, as seen in the transverse section, are of the ordinary parenchymatous type, varying in size from ·003 to ·00125 *. Some of these cells either contain isolated secondary cells within their cavities, or distinct primordial utricles, detached from their proper cell wall. Scattered amongst them also are some small vessels. In the longitudinal section (fig. 14) the cells of the medulla are seen to be of irregular shapes and sizes, but often narrow, elongated longitudinally, and square-ended. The vessels of the surrounding cylinder are generally of large size, ranging from ·007 to ·005 in diameter, and with strong, sharply defined walls; but at the inner and outer boundaries of the cylinder we have many small ones, not exceeding ·002, resembling, in this respect, some vessels which are scattered through the medullary parenchyma. On examining the longitudinal section (fig. 14) of this axial cylinder, we find that the vessels are of the usual barred type, and that the largest of them occupy the central parts of each cluster—a fact also shown by fig. 13. But the most remarkable feature of these vessels is further illustrated by figs. 15 & 16. In fig. 15 the dark, strongly marked walls of the vessels enclose densely packed masses of cells (*i*). The longitudinal section, made nearly through the centre of a single vessel (fig. 16), shows it to be similarly crowded with *tylose* throughout its entire length. The existence of this singular tissue in the vessels of recent plants has long been known to botanists, and Professor W. T. THISLETON DYER has described a similar tissue existing in a fossil Tertiary exogenous wood from Herne Bay and the Isle of Thanet †; but so far as I am aware this is the first recorded example of its appearance in a fossil Cryptogam, and especially in one from these ancient Carboniferous deposits. The fact of the existence of *tylose* at this remote period affords a striking example of the persistence of types of elementary tissue, additional to those which we already possess.

Bundle-sheath.—The axial vascular cylinder is enclosed in a somewhat irregular sheath of cellular tissue seen at figs. 13, *g* & 14, *g*. This consists of three or four rows of long, narrow, square-ended cells of variable length and about ·0005 in diameter; but interspersed amongst them are many scattered cells of larger dimensions, as shown in fig. 13.

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* As in the preceding Memoirs, all these measurements are recorded in the decimal parts of an inch.

† "On some Fossil Wood from the Lower Eocene," by W. T. THISLETON DYER, B.A., B.Sc., F.L.S., 'Geological Magazine,' vol. ix. no. 6, June 1872.

Cortex.—External to the vascular bundle-sheath we have a thick mass of ordinary parenchyma, figs. 4, *e*, 14, *e*, & 20, *e*, the cells of which are remarkably uniform, both in texture and size, their diameters chiefly ranging between ·005 and ·003. This parenchyma is invested by a thin outer layer about ·04 in thickness (figs. 4, *f*, & 2, *f*), composed of denser and darker-coloured cells.

Secondary bundle.—I have already remarked that, besides the large central vascular cylinder, each transverse section of the stem reveals two or more secondary bundles. Of these the largest and most conspicuous is the usually solitary one of this type represented in fig. 4, *c*; and in the subsequent figures the same bundle is indicated by the same italic letter. An enlarged representation of this bundle, as it appears in the section, fig. 4, is seen in fig. 17. Its outline is reniform. The greater part of the vessels, *g*, of which it is composed are nearly as large as those of the central cylinder; but those seen clustered together at each extremity of the transverse section, *g'*, are very much smaller than the others. This vascular bundle is surrounded by a sheath of narrow elongated cells (fig. 4, *g''*), identical with that investing the axial cylinder, and this, in turn, is enclosed within the ordinary parenchyma of the inner cortex (fig. 4, *c*).

The origin and destination of this bundle would be alike obscure were our study of it limited to a single section; but by following the ascending series of sections included in the figures 2–12 we obtain information on both these points. In the sections from figs. 2–8, *c*, we see but little change in the position and surroundings of the bundle; but in fig. 9 we find that the outermost cortical layer of the stem (fig. 9, *f*) is obviously bending inwards at two points, *f'*, *f'*, tending towards the enclosure and detachment of the bundle from the main stem. In fig. 10 this enclosure is complete. The bundle *c* is now the centre of what has every appearance of being the base of a leaf-petiole. In fig. 11, *c*, the bundle has been accidentally broken away in the preparation of the section; but at *f'* we find the petiole freeing itself from the central stem, whilst in fig. 12 the part of the stem, *f'*, from which the orientation took place is now encircled by the normal investment of outer cortex. In its original state the outer surface of the specimen exhibited several rounded areas not marked by defined cicatrices, but irregularly fractured, from which points similar petiolar extensions seem to have been broken off. Fig. 18 represents a section of one of these detached petioles enlarged 7½ diameters. I am indebted for this section to Mr. Butterworth, by whom it was prepared. Its central vascular bundle (*c*) is virtually identical in size and contour with that of fig. 17, whilst the inner (*e*) and outer (*f*) cortical layers are, in like manner, identical with those of the parent stem. The large rounded lacunæ in the inner bark are merely accidental results, probably of partial desiccation before the specimen was imbedded in its matrix.

Origin of the primary bundles.—On returning to the series of sections, we observe that at the point *b'* of fig. 6 the central vascular cylinder is undergoing a change. The arrangement of the tissues at this point is seen in fig. 19, which is an enlarged representation of the part of the central cylinder enclosed between the points fig. 6, *b'*, *b'*. At fig. 19, *b'* we find a cluster of the vessels bending outwards from their normal vertical course. In fig. 7, *b'** we find that a further outward extension of these divergent vessels has taken place. Fig. 20 is an enlargement of this part of the cylinder. In it we see that the vessels which bend outwards in the direction of the arrow through a part of their course (*b''*) are again bending upwards at *b'*, so as to be intersected transversely. In fig. 8, *b'* we find the bundle completely detached from the central vascular axis, and now it is evidently a bundle like *c* of fig. 2, only located on the opposite side of the stem†. In figs. 11 & 12 we have manifest evidence that this bundle *b'* is about to follow the course of its predecessor on the opposite side of the stem, since the outer bark (*f'*) is rapidly approaching to the conditions seen at *f'* in the figures 8 & 9.

Fig. 21 exhibits a somewhat oblique longitudinal section which passes through the central cylinder at *b*, and is nearly in the plane of the primary bundle *c*, the sheat of the latter being seen at *g*. This little section is important, since it shows that the ascending direction followed by this primary bundle is also followed by certain smaller ones yet to be described, as at fig. 21, *d*.

Where the vessels are about to leave the central cylinder to form one of the primary bundles just described, as at fig. 19, *b'*, several of my sections reveal a curious change in the structure of some of the elementary tissues. As already stated, the vessels composing both the central cylinder and the primary petiolar bundle are of the barred type so common amongst the Carboniferous plants. Fig. 22 exhibits a cluster of the tissues seen at fig. 19, *b'*, enlarged 62 diameters. At *b* we see *barred cells* which, yet further to the right of the section, pass into the ordinary barred vessels of the axis; but at *b'* we find similar cells, but with reticulate instead of barred walls, and at *b''* similar reticulated structures are being elongated, approaching the form of the ordinary vessels of the plant. This conversion of the barred into the reticulate type, the latter being a mere modification of the former, is merely a local phenomenon. I only detect it where the central cylinder is about to give off a petiolar branch, and here only in a very limited measure; no trace of reticulate vessels is to be found elsewhere, either in the central or the lateral bundles.

Secondary bundles.—In addition to the large vascular bundles supplying the petioles we find in several of the sections smaller bundles, some of which are given off directly from the central cylinder, others from the primary petiolar bundles. Instances of the former of these orientations are found in figs. 3, 5, 7, 10, & 12, and an example of the latter occurs in fig. 5. In fig. 3 a bundle is separating from the upper part of the cylinder at *d*. In fig. 4, *d*, we discover this same bundle completely detached from the central axis and proceeding direct to the periphery of the cortex. In the longitudinal section, fig. 21, we have, at *d*, a corresponding bundle. In both these examples these *secondary* bundles obviously *ascend* through the stem as they proceed outwards, in exactly the same way as the larger *primary* petiolar ones do. Though following the same course, the former are very much smaller, both in their aggregate diameter and in the size of their component vessels, than the latter are. The entire series of sections appears to exhibit them of two sizes. In figs. 4, *d*, 7, *d*, & 8, *d*, each bundle has a diameter of ·032, whilst in figs. 5, *d*, 10, *d*, & 12, *d*, it is only ·01 in thickness. It is another notable point in these eleven consecutive sections, that whilst we only find two of the large primary bundles (*d*) emanating directly from the central cylinder, we have certainly five of these smaller ones doing so. In fig. 7, as shown in the enlarged representation, fig. 20, *d*, the secondary bundle is seen springing almost from the central axis simultaneously with the primary one *b'*.

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* The arrows of the figures 7 and 20 indicate the directions in which these two figures should be turned to place them in identical positions.

† In figs. 8 & 9, 10, 11, & 12 the sections have been slightly twisted round to make them fit more symmetrically upon the plate; hence, whilst the vessels *b'* of fig. 7 point downwards towards the *lower* right-hand corner of the section, in all the subsequent sections referred to the bundle which these divergent vessels have formed is situated directly to the right of the cylinder.

But we further find secondary bundles arising from the primary petiolar ones. We have one such originating at *d'* from the primary bundle, fig. 5, *c'*. In fig. 6, *d'*, we find that this secondary bundle has reached the periphery of the stem, and, as seen in fig. 24, *d'*, which represents this bundle and its surroundings enlarged 18½ diameters, it is now the centre of a distinct organ somewhat similar to fig. 18, but having little more than one third the mean diameter of the latter section. Traces of this appendage are still seen in figs. 7, *d'* & 8, *d'*, but in fig. 9 it has wholly disappeared. The correctness of this description depends upon the identification of the bundle, fig. 6, *d'*, with fig. 5, *d'*. But on recurring to figs. 2, 3, & 4 we find a curious bundle unaccounted for at *d* in each of these sections. It must be remembered that fig. 2 is the lowest of this series of sections, and that as we ascend in the series we ascend in the stem. Observing that the small bundle, fig. 2, *d*, is further removed from the primary bundles *c* than in the sections figs. 3 & 4, it appears possible that these latter bundles may be identical with *d* in fig. 5. If this be the case, we have here an instance in which a secondary bundle *descends* as it passes outwards through the bark, instead of ascending, as we have seen to be the case with the small bundles given off from the central cylinder. Such an arrangement would correspond exactly with that so common amongst such living ferns as *Aspidium Filix Mas*, where the bundles of vessels supplying the adventitious roots spring from the petiolar bundles, subsequent to the separation of the latter from the true axial bundles of the stem. The discovery of fresh specimens can alone settle this point.

Two questions still remain to be answered, viz., what is this stem? and what is the morphological significance of its various parts?

As I have already observed, M. RENAULT has described two stems which have the axial vessels arranged as in the subject of the above description, an arrangement which differs very widely from what is seen in any known ferns, recent or fossil. The detached bundles of fibro-vascular tissue seen in recent ferns, whether of the herbaceous or of the arborescent type, exhibit no arrangement exactly corresponding to the closed cylinder seen alike in my fossil and in the *Zygopteris Brongniarti* and the *Anachoropteris Decaisnii* of M. RENAULT. In all these three plants the vessels form a distinct vascular cylinder which encloses a medullary tissue, reminding us of the simpler types of Lepidodendroid stems rather than of true ferns. Yet there seems to be no reason for doubting that M. RENAULT'S plants are true ferns; and if so, my specimen corresponds with them too closely in all its essential features to be separated from *Anachoropteris* and *Zygopteris*. Assuming that this is the case, what are the lateral organs into which my stem obviously branched? My primary divergent bundles, indicated in all the figures by the letter *c*, went either to secondary branches or to petioles. The analogies of M. RENAULT'S plants, as well as the general aspect of fig. 18, indicates that the latter was the case; but if so, what were the smaller secondary bundles indicated by the letter *d*? Springing directly from the central axis, and *ascending through the cortex* like the primary bundles, their direction seems to indicate that they also went to petioles, but to some of smaller size than those to which the larger and less numerous primary bundles were distributed. In his recently published memoir on *Botryopteris forensis* (Annales des Sciences Naturelles, 6^e série, Bot., tome i. pl. 8. fig. 1) M. RENAULT has figured similar secondary bundles, given off, as in my plant, both from the central vascular axis and from the primary vascular petiolar bundle. The learned author does not hesitate to regard them as supplying rootlets. These moot points can scarcely be finally settled until we meet with additional examples of this curious stem.

So distinctly characterized a stem requires an appropriate name. In my last memoir I endeavoured to avoid a needless multiplication of generic terms by grouping together a number of unidentified petioles under the common name of *Rachiopteris*. But we have now three examples of these petioles identified with stems, all of which latter exhibit a common type of internal organization, though their petioles display differences in the arrangement of their vascular tissues. But the latter fact constitutes no reason why the three plants should not be united in one common genus. In my previous Memoir* I pointed out that such differences of structure in the petioles did not prevent *Adiantum trapeziforme* and *A. cuneatum* from being placed in one genus, or *Pteris umbrosa* and *P. aquilina* in another. Neither should it prevent M. RENAULT'S *Zygopteris Brongniarti*, *Anachoropteris Decaisnii*, and my new plant from receiving a common generic name. But a new difficulty now springs up. CORDA assigned each of his two generic terms, *Zygopteris* and *Anachoropteris*, to special forms of petioles. Hence if we select either of these names to represent the triad of plants in question it can only be done by giving an entirely new definition to an old generic name, and one, moreover, in the case of *Zygopteris*, that is still required for several petioles, such as *Z. Lacattii* and *Z. bibractiensis*, of which the stems have not yet been found. Hence, though *Zygopteris Brongniarti* must be included in the new genus, it seems undesirable to employ separate generic names for the three stems.

Under these circumstances it appears to me to be wiser, for the present, to comprehend all these fragmentary stems and petioles in a common group, such as *Rachiopteris*, without pretending to assign to that term a true generic value, and then to subdivide the group into subsections, according as the specimens appear to have features in common, such as that of *Zygopteroides*, *Stauropteroides*, &c. By this plan we avoid the absurdity of creating numerous meaningless genera, each one of which is only represented by fragmentary parts of plants. Hence I would designate the plant which I have just described *Rachiopteris corrugata*, and include it, along with M. RENAULT's *Zygopteris Brongniarti* and *Anachoropteris Decaisnii* *, in the subgroup of *Anachoropteroides*, the distinguishing features of this group being the possession of a central vascular axis which forms a virtually closed cylinder surrounding a cellular medulla—two features in which these stems of Ferns approach very closely to those of the type of *Lepidodendron* represented by *Lepidodendron Harcourtii*. In the adoption of the above plan of classification I have benefited by some valuable hints from Dr. HOOKER, who kindly gave me the advantage of his vast experience in the study of systematic botany.

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* Phil. Trans. vol. 164. part 2, p. 676.

It has long been noticed as a remarkable fact that whilst the Oolitic ferns of the Gristhorpe shales so frequently exhibit sori attached to their fronds, ferns with sporangia *in situ* are rare in the true Coal-measures. BRONGNIART long ago figured a solitary leaflet of *Neuropteris flexuosa* which affords indications of sori upon its surface (Vég. Foss. pl. 65. figs. 3, 3 A, & 3 B). CORDA figured two examples, under the names of *Senftenbergia elegans* and *Hawlea pulcherrima* †, in which these conditions exist; and more recently M. RENAULT has described two other examples under the names of *Botryopteris dubius* and *B. forensia* ‡, in which also the sporangia are found attached to a branching axis. One of these species was from Autun, the other from St. Etienne. Prof. SCHIMPER has figured a fertile frond of *Palæopteris Hibernica* (Traité de Pal. Végét. vol. i. pl. xxxvi.). Mr. Bailey has represented the same fern in his 'Figures of Characteristic British Fossils' (pl. xxviii. fig. 1), and, still more lately, Mr. CARRUTHERS has figured the same plant with fertile pinnules in the 'Geological Magazine,' vol. ix. No. 2. The latter Memoir also contains figures and descriptions of some detached sporangia from the Oldham deposit, which its author believes to be of the type found in the recent Hymenophylleæ.

During my long-continued researches I have failed to discover a solitary fern-frond in the Coal-measures of Lancashire, Yorkshire, or Durham in which sori were to be seen *in situ*, and in my large collection of sections I have met with extremely few traces of detached sporangia. Here and there a fragment of an annulus has been observed, but too imperfect for identification with any special type §. Mr. BUTTERWORTH has been more fortunate in discovering the fine specimen represented in fig. 25. Its mean diameter is nearly ·03. Its annulus (*a*) is obviously of the common type found amongst the Polypodiaceous ferns. *b* is clearly part of the peduncle; hence the annulus has been a vertical one of the usual form, differing in this respect from the examples described by Mr. CARRUTHERS. The interior of the sporangium is crowded with small spores, some of which are spherical, others apparently angular; but this latter appearance may merely result from defective mineralization. Fig. 26 represents one of the spherical spores enlarged 320 times. Their actual diameter is about ·0018. This specimen is from the Oldham nodules. We have no clue to the frond to which it belonged.

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* Annales des Sciences Naturelles, 5e série, Bot., t. xii. (Cahier No. 3).

† Flora de Vorwelt, Taf. lvii.

‡ Annales des Sciences Nat., 6e série, Bot., tome i. pl. 10.

§ This remarkable fact indicates that the fossil fronds have been exposed for a long time to the action of water before they became buried in silt and mud. At the same time, remembering how common fern-fronds are in the coal-shales, and what myriads of sporangia they must have set free, it is strange that we do not find detached ones more abundantly than is the case. (Since the above remarks were penned, I have obtained additional examples of such sporangia from Messrs. BINNS and SPENCER, of Halifax.—June 2, 1877.)

Figs. 27 to 32 exhibit the fructification of a Fern of an altogether different type. Figs. 27 & 28 represent lateral views of two sporangia of which *a* indicates the point of attachment. No peduncle is seen in any of the specimens, hence I presume that these sporangia have been sessile ones. They are slightly pyriform in shape, and their walls consist of large, strongly defined prosenchymatous cells. Fig. 29 is a transverse section of what appears to have been the upper part of the sporangium, and has passed through a horizontal annulus, whilst the internal cavity contains a small number of large spores. Fig. 30 is also a transverse section, but apparently made yet nearer to the upper extremity of the sporangium *above* the termination of the annulus. One solitary spore is seen within the internal cavity. The mean diameter of the two specimens, figs. 28 & 29, is ·02.

There can be no doubt that in these sporangia the annulus formed a conspicuous cap near the upper or free extremity of each sporangium. The spores are much larger than those of fig. 25, as is seen in figs. 31 & 32, which are drawn to the same scale as fig. 26. Each spore is about ·0023 in diameter, and consists of an outer cell wall (figs. 31 & 32, *a*) and an inner or primordial utricle (*b*). These sporangia are apparently of the type seen in the modern families of Gleichenieæ and Schizæeæ, but especially approaching those of the latter family. Somewhat similar sporangia have already been figured by CORDA associated with his *Senftenbergia elegans*, and most probably also in the case of his *Hawlea pulcherrima*. Speaking of the former plant, M. BRONGNIART says, "Mais quoique ce genre fossile se rapproche beaucoup du *Mohria*, il en diffère, non seulement par la forme générale de la fronde, mais surtout par les capsules, dont l'anneau élastique est formé, dans le *Mohria*, d'un seul rang de cellules linéares, radiées, tandis que dans le *Senftenbergia*, d'après M. CORDA, il est composé de plusieurs rangées régulières de cellules"[*]. The peculiar arrangement characteristic of *Mohria* and other allied genera, but which is lacking in *Senftenbergia*, is precisely that which is seen in my examples[†].

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[*] 'Tableau des genres des Végétaux fossiles.'

[†] I am indebted to Professor OLIVER, of the Kew Gardens, for examples of Gleicheniæ and Schizææ, with which he supplied me for the purpose of comparison with my fossil specimens. This is only one amongst several instances in which my applications for assistance to the Kew establishment have received the most prompt and liberal responses.

M. Corda's plants were obtained from the Bohemian coal-field. It is now probable that the remarkable group of the Schizæeæ is represented amongst our British Carboniferous ferns.

The next plants to which I would call attention bring us to one of the most debatable and earnestly debated of all the problems which the study of the Coal-plants has suggested. They are the Coniferæ, or, to employ a more comprehensive term, the Gymnospermæ. In his 'Prodrome d'un Histoire des Végétaux Fossiles,' published in 1828, M. Brongniart says of the Gymnospermous Phanerogams, "Aucune plante du terrain houiller ne paroît se rapporter à cette classe, à moins que quelques-unes des plantes que nous avons placées parmi les Lycopodes ne fussent des Conifères"[*].

In 1838 Witham published his well-known memoir "On the Internal Structure of Fossil Vegetables found in the Carboniferous and Oolitic Deposits of Great Britain." In this he described a number of plants which he arranged in two genera, *Pitus* and *Pinites*. The former genus was one of his own institution[†]. The latter he adopted from the authors of the 'Fossil Flora of Great Britain,' who, in their first volume (p. 9) figured and described portions of the celebrated Craigleith Tree under the name of *Pinites Withami*. Witham also inclined to the belief that his *Anabathra pulcherrima* was a plant of Coniferous affinities. I have already pointed out, in a previous memoir[‡], that this latter plant was undoubtedly a Diploxyloid *Lepidodendron*. As to the remainder of Witham's plants I have tried in vain to form an accurate conception of their true character. Unfortunately Witham was wholly ignorant of the distinction between two very different classes of markings seen on the surfaces of prosenchymatous fibres, cells, and vessels. He evidently confounded the reticulations, which are merely deposits within and in close union with the cell-wall, and which are mere modifications of true spiral tissues, and the *disks* seen in Conifers and Cycads, and which, as is now well known, are lenticular spaces connecting two contiguous fibres. Accurately made tangential sections, in which the true medullary rays are intersected at right angles to their course, also exhibit sections of these lenticular cavities, appearing like very delicate vertical lines of chain-work. Of this eminently characteristic difference between two varieties of structure Witham knew nothing, as is demonstrated by his typical, tangential sections of recent examples represented in Plate 2. figs. 3–6 & 9, in which no traces of these intervascular structures are shown. I have no doubt that some of his supposed Conifers belong to the groups of plants which I have described in my fourth memoir[§] under the names of *Lyginodendron* and *Heterangium*, which I believe to be Cryptogamic genera[||], whilst others doubtless do belong to the group which palæontologists

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[*] *Loc. cit.* p. 175.

[†] *Loc. cit.* p. 38.

[‡] Phil. Trans. 1872, pp. 299 & 309.

[§] Phil. Trans. 1873, p. 379 *et seq.*

[||] Even Lindley and Hutton had strong doubts respecting the coniferous character of Witham's stem, which in their 'Fossil Flora' they designated *Pinites Withami*. They say, "Notwithstanding the great similarity between the transverse sections of this wood and those of recent Coniferæ, and notwithstanding the total absence of ducts in what seems to have been a tree having an exogenous structure, yet that as the very remarkable still regard as the nearest relatives of the modern Conifers that the Coal-measures have yet furnished.

In September, 1851, I laid before the Literary and Philosophical Society of Manchester a memoir "On the Structure and Affinities of the Plants hitherto known as *Sternbergiæ*." Dr. DAWSON, of Montreal, had previously shown* that some fragments of wood "much resembling the wood of the Coniferæ" had internal "stony casts having irregular and often large transverse markings." In my memoir referred to I arrived at the conclusion that *all* the Sternbergian specimens hitherto found in our British Carboniferous deposits were casts of the internal pith-cavities of stems and branches belonging to ENDLICHER'S genus *Dadoxylon*, a conclusion which more than twenty years of further research has thoroughly confirmed. CORDA, in his classic volume, had previously figured and described, under the name of *Lomatophloios crassicaule*, a Lepidodendroid plant with a Sternbergian pith-cavity, and Dr. DAWSON believes that a similar pith is characteristic of some Sigillarian stems which he has discovered in the Coal-measures of the Dominion; but British observers have hitherto failed to detect any similar Lepidodendroid or Sigillarian forms in our Coal-measures. I repeat that every fragment of *Sternbergia* hitherto found in our Islands, the surrounding tissues of which show satisfactory indications of structure, belongs to the genus *Dadoxylon*.

In 1849 M. BRONGNIART published, in the 'Dictionnaire Universel d'Histoire Naturelle,' his very important "Tableau des genres de Végétaux Fossiles." On comparing his views as enunciated in this memoir with those embodied in his earlier 'Prodrome,' we find that they underwent a vast change, so far as the question of Carboniferous Gymnosperms is concerned. In his later publication he included in that group *Calamodendron*, *Asterophyllites*, *Sphenophyllum*, *Annularia*, the entire group of *Sigillariæ* with their Stigmarian roots, the *Diploxylons* of CORDA, the *Medullosæ* of COTTA, and a number of other less important plants. It is not necessary to say that I altogether reject these arrangements. I have, in previous memoirs of this series, given my reasons for believing that the *Calamodendra* are all Equisetacean, that *Sigillaria* and *Diploxylon* are Lepidodendroid, that *Asterophyllites* and *Sphenophyllum*, if not Lycopodiaceous, cannot possibly be regarded as Gymnospermous, and that *Medullosa* is certainly a Marattiaceous Fern. But M. BRONGNIART also included in that Gymnospermous group several of the recently established genera of Carboniferous plants, such as the *Pinites medullaris* and the *P. Withami* of LINDLEY and HUTTON, which he placed in his genus *Palæoxylon*. In like manner he placed the *Pinites Brandlingi* in ENDLICHER'S genus *Dadoxylon*, and the *Pitus primæva* and *P. antiqua* of WITHAM in ENDLICHER'S genus *Pissadendron*. These and other supposed fossil coniferous woods BRONGNIART threw into two groups according

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organization of the walls of the woody tissue of recent Coniferæ does not exist in this fossil, but is supplied by another kind of structure of an equally unusual nature, the inference that this tree belonged to the coniferous tribe cannot be considered altogether just."—*Loc. cit.* p. 12.

* Proceedings of the Geol. Soc. London, No. 6, Jan. 1846.

to whether the individual medullary rays, as seen in vertical tangential sections, consisted of one or more vertical series of cells. **Brongniart** having unhesitatingly accepted all **Witham**'s descriptions and figures, his conclusions respecting the British Carboniferous Conifers are as untrustworthy as I have shown those of **Witham** to be. Notices of various Carboniferous Conifers occur in Dr. **Dawson**'s numerous and valuable contributions to the history of Cryptogamic vegetation; but he more especially grapples with the complicated definitions of previous authors in his "Report on the Fossil Plants of the Lower Carboniferous Millstone-Grit Formations of Canada." In p. 15 of that report, published in 1873, after indicating the way in which various genera of supposed coniferous plants had been distributed and redistributed by preceding writers, he describes and figures the *Dadoxylon antiquius*, which species he had previously described in his 'Acadian Geology;' a second species, also described in the 'Acadian Geology,' appears under the name of *Dadoxylon Acadianum*; a third is the *D. annulatum*; and a fourth is the *D. materiarium*, all of which are in like manner described in the writer's 'Acadian Geology.'

In August, 1869, I published a brief memoir in the 'Monthly Microscopical Journal,' in which I endeavoured to distinguish those stems in which the vessels were merely reticulate modifications of scalariform tissue, from those in which vessels or fibres exhibited true bordered pits or disks, such as are seen in the *Coniferæ* and other plants. In making an examination of recent stems, in connexion with that inquiry, my attention was arrested by some peculiarities in the vascular tissues of the *Cycadeæ*, a further study of which led me to the following conclusion:—"I have for some time been convinced that the discigerous vessels in *Cycas revoluta*, usually supposed to be of a coniferous type, were in some measure modifications of scalariform tissue. I have now found numerous vessels from the above plant which render the fact certain, since they exhibit discigerous tissue at one end of the vessel, whilst it becomes scalariform at the other extremity. My views on this point, when promulgated in private correspondence with some botanical friends, were at once rejected by them; but there is no reason for questioning their correctness. I was not aware, however, when I came to this conclusion, that I had been anticipated by the late Mr. **Don**, in a paper which he read before the Linnean Society in 1840. The question is of some importance, since it affects the possibility of a glandular coniferous fibre being developed out of a reticulated one"[*]. The true nature, and still more the origin, of these disks seen on coniferous fibres was exceedingly obscure up to a very recent period. Even **Henfrey**, in his 'Elementary Course of Botany,' speaks of "the existence of a lenticular cavity *between the contiguous outside walls* of pitted cells, as in Coniferæ (fig. 480)," and in the figure referred to he represents this lenticular cavity as having walls of its own, in addition to those of the two vessels between which it is located. It is obvious that these views respecting the nature of the bordered disks made the passage of reticulated into discigerous fibres very difficult of apprehension. But the appearance of **Sachs**'s

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[*] Monthly Microscopical Journal, August 1, 1869, p. 69.

'Lehrbuch der Botanik' altered the position of this question. SACHS showed (*loc. cit.* p. 25) that what is essential in the formation of these pits is primarily due to a mere variation in the deposition of the lignine *in the interior* of the cell or fibre, corresponding exactly in this respect with what takes place in the formation of a scalariform or spiral vessel, the only material difference being found in the location of these internal deposits and their greater or less close adhesion to the cell-wall within which they are formed. This is a very different thing from what it was believed to be when it was supposed that the lignine was deposited *inside* the cells or vessels, whilst the disks depended upon something *external* to their walls. In the latter case the gradual transition from the one form to the other was unintelligible. *Now*, on the contrary, nothing is more simple.

This physiological inquiry is not foreign to the subject of this memoir, but forms an essential part of it, since it gives us some clue, helping us to determine what may and what may not have been Coniferous plants. It affects the position in which special genera should be located, besides having a very important bearing upon the general question of evolution. Though at present unable to see my way to the acceptance of this doctrine in connexion with the origin of extinct plant forms, I am anxious to place on record all the facts which my studies in palæo-botany supply which appear to support that doctrine. Hitherto the difficulty of explaining the transition from the ordinary types of fibro-vascular cells and vessels to that of the discigerous types has been one very great hindrance to my accepting the doctrines of the evolutionists in reference to fossil plants; this one difficulty, at all events, has now been removed. But its removal is also an important fact bearing upon another branch of the present inquiry.

M. BRONGNIART, Dr. DAWSON, and Professor NEWBERRY have long held the view that many of the *Sigillariæ* and a large group of allied stems were those of Gymnospermous plants. The two first distinguished palæo-botanists also believe that many of the forms of Calamites, which they recognize under the name of *Calamodendron*, are also members of the same Gymnospermic family. But these observers have arrived at their conclusions on somewhat different grounds. M. BRONGNIART has done so from his accurate recognition of their exogenous structure and mode of growth—features which, as I think I have demonstrated, are to be found in many indisputably Lepidodendroid plants. Hence, I cannot accept M. BRONGNIART's conclusions as a legitimate deduction from the evidence upon which the distinguished Frenchman made them rest. Dr. DAWSON has told us, in a succession of memoirs, that he has arrived at very similar conclusions to those adopted by M. BRONGNIART, but through a somewhat different line of argument, the chief points of which are prominently set forth in his memoir "On the Structure and Affinities of *Sigillaria*, *Calamites*, and *Calamodendron*," published in the Quarterly Journal of the Geological Society of London for May, 1871. In that memoir he describes a stem in which the pith was Sternbergian, whilst the inner part of the vascular cylinder "consists of scalariform tissue, passing towards the outer surface into pseudo-scalariform, reticulated with pores, and discigerous." The accuracy of this description of a Sigillarian stem appeared to most British botanists so little probable and so little in accordance with recognized views as to the relations of scalariform to discigerous tissue that it failed to meet with acceptance. The chief difficulty which stood in the way of my adopting it was the entire absence from our British deposits, in which structureless *Sigillariæ* are so common, of any stems with a structure corresponding to it; but it must be admitted that, as yet, I have obtained no trustworthy specimens which I could, *without possibility of doubt*, affirm to be the true vascular axes of *Sigillariæ*. Portions of the bark are, as I have shown in one of my previous memoirs, sufficiently common, whilst decorticated vascular axes of Diploxyloid types are also far from rare; and seeing that many of these latter approach so near to the Sigillarian stem figured by BRONGNIART as well as, in all essential features, to that of *Sigillaria spinulosa* described by MM. RENAULT and GRAND'EURY*, it appeared to me extremely probable that some of these Diploxyloid axes belong to true *Sigillariæ*. In no one of these have we found a trace either of a Sternbergian pith or of the discigerous fibres in the exterior of the vascular axis which Dr. DAWSON has described as existing in his American examples. Nevertheless we must not conclude from this that Dr. DAWSON is in error, either in the accuracy of his description or in his belief that the stems described are those of true *Sigillariæ*, even though we may be convinced that these plants are Cryptogams and not Gymnosperms, as Dr. DAWSON thinks probable†. That Sternbergian piths *may* occur in Lycopods, as well as elsewhere, is shown in the case of CORDA's *Lomatophloios crassicaule*; and, if SACHS is right in his explanation of the real structure and origin of the bordered disks of coniferous fibres, there is no morphological or physiological reason why the barred vessels forming the *inner* zones of the axis of a Lepidodendroid plant should not gradually change towards its periphery, first into what Dr. DAWSON terms pseudo-scalariform tissues, and still more externally into discigerous ones. But even admitting all this to be true in the fullest sense that Dr. DAWSON would demand, it does not bring me one bit nearer to the admission of *Sigillariæ* amongst Gymnospermous plants, or to their separation from the *Lepidodendra*. With the bark of our common ribbed Lancashire *Sigillariæ* we are now thoroughly familiar, and its very remarkable structure is as identical with that of the *Lepidodendra* as it can possibly be, whilst it is as different as possible from any known Gymnosperms, recent or fossil. In one of his latest popular works, speaking of *Sigillariæ*, Dr. DAWSON says, "Some regard it as a Gymnosperm, others as a Cryptogam. Most probably we have under this name trees allied in part to both groups, and which, when better known, may bridge over the interval between them"‡. Dr. DAWSON has recently forwarded to me his latest definition of *Sigillaria*, with permission to make use of it. It is as follows:—

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* Mémoires présentés par divers Savants à l'Académie des Sciences de l'Institut National de France, tome xxii. no. 9.

† In a letter to the author, dated April 14, 1876, Dr. DAWSON says, "I have never held that any *Sigillariæ* are Conifers, but only that *some Sigillariæ* in their tissues closely resembled the Cycads and were Gymnosperms. I have also held, on the evidence of form of scar and leaf, that some so-called *Sigillariæ*, especially those of the *Clathraria* type, are of humbler structure and allied to Lycopods."

‡ 'The Story of the Earth and Man,' p. 124.

Genus SIGILLARIA.

Roots stigmaroid.

Trunk Sternbergia pith. Double woody cylinder pseudo-scalariform within and discigerous without, with medullary rays and oblique pseudo-scalariform leaf-bundles. Inner bark thick, cellular, with many bundles of prosenchymatous tissue. Outer bark dense, cellular*.

Leaf-bases hexagonal or elongated, or confluent on a vertical ridge, placed in vertical rows, except in young branches.

Leaf-scars hexagonal or shield-shaped, with three vascular scars; the two outer largest.

Fruit-scars in transverse rows or bands, each with a central vascular scar.

Fruit Trigonocarpon, borne in racemes attached to the fruit-scars.

If Dr. DAWSON is correct in making this last statement in reference to *Trigonocarpon*, all doubt as to the Gymnospermous character of *Sigillaria* would be at an end, because we cannot possibly conceive of that seed belonging to any Cryptogamic plant. But I have yet seen no evidence whatever, either in his memoirs or in those of Professor NEWBERRY, who entertains the same opinions as Dr. DAWSON does, that any such relationship exists. Under these circumstances, in the subsequent pages I must include *Trigonocarpon* amongst the Gymnospermous plants, but must exclude the *Sigillariæ*, and still regard the latter as Lepidodendroid Cryptogams.

In my original memoir on the *Sternbergiæ*, published in the 'Transactions of the Literary and Philosophical Society of Manchester' in 1852, I described a small branch of a *Dadoxylon* from Coalbrookdale, for which I was, many years ago, indebted to Professor PRESTWICH. This specimen continues to be incomparably the most perfect example of its class yet discovered; hence I have felt it necessary carefully to restudy my sections of it, and now proceed to give the results of this renewed examination.

The specimen has been a young branch about half an inch in diameter. In this respect it corresponds with many that we find in the Oldham deposits; but the interest of the specimen lies in the exquisite manner in which all the tissues are preserved.

The medulla (figs. 33, *a*, & 35, *a*) has a diameter of about ·3, and consists of a regular, but rather thick-walled, parenchyma, the cells of which are about ·0025. In the vertical section (fig. 35) these cells are seen to be somewhat depressed vertically, their horizontal diameter being greater than their vertical one. They also exhibit a tendency to arrange themselves in vertical lines. This medulla is an undivided mass for a thickness of about ·03, but more internally it separates into innumerable thin laminæ, which are very conspicuous in the broken fragments of the specimen, but of which I have found it impossible to prepare microscopic sections, owing to the extremely friable nature of the ferruginous matrix occupying the interlaminar spaces. This matrix appears to consist of small botryoidal granules (some infiltrated) of ferruginous oxide, which have loosely and imperfectly filled the cavities separating the medullary disks. This explanation is the more probable since the entire specimen was originally enclosed in a nodule of the ordinary Clay Ironstone so common in the Coal-measures of Coalbrookdale. There is no doubt whatever that these cellular laminæ are identical with those of the ordinary forms of *Sternbergia*, only more closely aggregated than is usual with specimens of larger size.

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* Since the above description was written, Dr. DAWSON has discovered a *Sigillaria* with a true Diploxyloid axis, corresponding exactly with those which I have already described and with the *Anabathra* of WITHAM. This discovery would probably lead him to modify some of the above definitions.—June 18, 1877.

The ligneous part of the stem is a cylinder of wood-fibres (figs. 33 & 35, *b*), the thickness of the cylinder from the medulla to the bark being about ·05. At its innermost surface, *b'*, we find a few very small, but distinct, barred vessels occupying the position of a medullary sheath, which element of exogenous stems they apparently represent: three of these vessels are enlarged in fig. 37. In the transverse sections the vessels are arranged in very regular radiating lines. They have a mean diameter of ·0012, which corresponds with those of *Salisburia adiantifolia* and *Araucaria Braziliensis*; but their walls are thinner than in the corresponding twigs of either of those living Conifers. These vessels terminate in fusiform extremities. Their length varies, but many of them are not more than ·012; others are somewhat longer. These proportions show that the tubes do not differ materially from the ordinary woody prosenchyma of living Conifers. The magnified representations (figs. 38 & 39) show that the two surfaces *which are parallel with the medullary rays* are marked with very regular geometric areolæ, each of which has a mean vertical diameter of ·0005. Their transverse diameter varies. Sometimes a single row runs down the centre of each fibre, each areole then stretching across almost the entire diameter of that fibre. In others, as in fig. 39, which represents their ordinary arrangement, we have two such rows. Occasionally, as seen in fig. 38, there are three vertical series of these areolæ. On turning to the tangential sections, fig. 40, we obtain in the beaded lines, *g*, *g*, clear proof that these areolæ are not those of ordinary reticulated fibro-vascular tissue, but true bordered pits, to the formation of which, as explained by SACHS, reference has already been made*. In the plant under consideration there is no central aperture in each of these areolar pits, such as is usually seen in living Coniferous plants; SACHS's explanation of their origin supposes that secondary deposits are formed within each fibre, but that there are thin circular spaces within each fibre in which the deposit does not rest upon the primitive cell-wall, but gradually rises up from it, like a circular dome, in the centre of which there remains a small, unclosed aperture, like that left for the reception of a skylight in a dome-shaped roof.

SACHS further shows that similar deposits take place at exactly corresponding points on the cell-walls of two contiguous fibres. Such growths would form two opposed semi-lenticular or plano-concave cavities, with the two thin coalesced cell-walls separating them. These latter become absorbed in living plants, so that the two plano-concave spaces become converted into one double concave one. In figs. 38 & 39 we see that no central aperture exists in each areole. Here nature has set the example to a builder who gradually raised the walls of his dome, but who, instead of leaving a central aperture for a circular skylight, built up his brickwork to the very keystone of the closed arch. So far as my specimens throw light upon this physiological feature, most of our Lancashire *Dadoxylons* have been constructed upon this type with imperforate pits.

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* Lehrbuch der Botanik, pp. 26, 27.

All the sections exhibit medullary rays. These are radially disposed lines of mural cells (figs. 35 *f*, 38 *f*), which cells are often cubical, having a diameter of from ·00125 to ·0007. In the tangential section (fig. 40) we see that these cells are arranged in single vertical lines. The number of cells in each vertical series varies from one (fig. 40, *f'*) to fifteen, such variable numbers being distributed indiscriminately over each section. Whilst the specimen under examination is remarkable for the preservation of the tissues just described, it is especially so in the case of the bark, which is unequalled by any other example of a Carboniferous *Dadoxylon* that I have met with; at the same time some of its outermost tissues are most probably wanting. The entire bark, as it now exists, is about ·05 in thickness, and is separable into two very distinct portions—an inner and an outer one. In the transverse section (fig. 34) the inner bark, *d*, appears as an indistinct tissue, in which traces of small cells, or of the orifices of oblique tubes, are visible. On the other hand, the outer layer (fig. 34, *e*) consists of a strongly marked form of parenchyma composed of cells of various sizes, the majority of them being about from ·002 to ·0025. On turning to the vertical section (fig. 36) we see that the inner bark, *d*, contains numerous parallel longitudinal lines, which are evidently the thin walls of tubes in which I can occasionally trace transverse septa. In all probability some of these latter are the representatives of *vasa propria*. They appear to have a diameter of from ·001 to ·0006. Some of these apparent tubes may be resin-canals. Whatever their physiological nature, the abrupt transition from them to the outer bark, as seen in the same section, is very marked. The latter tissue (fig. 36, *e*) is composed, as I have just observed, of coarse parenchyma, the cells of which are arranged in irregular vertical lines which tend somewhat outwards as the linear rows ascend through the stem.

Traces of divergent vascular bundles in this *Dadoxylon* will be considered after examining some similar organs in other specimens.

Fig. 41 is a vertical section of a specimen for which I am indebted to Mr. BUTTERWORTH. It consists wholly of the Sternbergian medulla of a *Dadoxylon*, and is drawn of the natural size of the original. At its upper part it exhibits extremely well the regular arrangement and unbroken continuity across the medullary cavity of the successive layers of the medulla. When I published my original memoir on *Sternbergia* I was uncertain whether or not these medullary disks were perfectly continuous, or whether they were perforated along the centre of the medullary axis. It is now certain that they were continuous. This specimen is further peculiar because of the two thick lateral masses of undivided pith (*a*, *a*), which, in this specimen, has evidently formed a rather thick layer of parenchyma lining the inner surface of the ligneous cylinder, the more central portion being split up into the characteristic Sternbergian disks. As we have already seen, this is the condition existing in the small Coalbrookdale specimen; but it is not the usual state of the pith in larger *Dadoxylons*, in which the Sternbergian laminæ are only enclosed within a very thin cylinder of undivided medullary parenchyma.

Fig. 42 is a transverse section of a large branch of the natural size. The medulla has been three quarters of an inch in diameter, and the entire stem, exclusive of its bark, was, when perfect, 2½ inches in diameter, the ligneous cylinder having a maximum thickness of nearly an inch. Though the woody wedges composing this cylinder have been disturbed by mineralization, it is obvious that the medullary extremities of the larger ones projected into the medulla, as is so commonly the case in the branches of living Conifers (*e. g. Araucaria imbricata*), giving to the transverse section of the pith a stellate outline, large pointed rays of the latter being prolonged outwards between the convex extremities of the wedges. This specimen, for which I am indebted to JOHN AITKEN, Esq., of Bacup, came from the Halifax Bullion or Ganister bed at South Owram. The matrix is full of small Goniatites. The longitudinal section (fig. 43) of the above branch displays some peculiarities. Thus the medullary layer lining the vascular cylinder is very thin, soon breaking up into thin disks, but which are very regularly thickened at their circumferential portions; hence, in the vertical sections, each disk appears as a pillar standing upon a rapidly swelling base (fig. 47, *a*), which latter rests upon the interior of the woody cylinder (*b*). Then the laminæ of the several disks, instead of extending straight across the medullary cavity, subdivide into two, one half joining the corresponding half of the lamina above, and the other half being united in a similar manner to the lamina below. Thus there is formed a central pile of lenticular cavities, *a*, which alternate with a more marginal series of lenticular rings, *a'*, *a'*.

Fig. 44 is a transverse section of a small branch from one of the bullions above the two-foot coal at Oldham; and fig. 45 is a vertical section of the same specimen. Its diameter is half an inch, of which the *medulla* occupies nearly ·19. This latter tissue not only divides into the usual disks, *a*, but each one of these again exhibits the strongest tendency to split up into numerous yet thinner layers, each one of which consists apparently of a single horizontal layer of cells*. It is obvious that in this instance the medullary cells have arranged themselves in successive vertical series of horizontal layers, and thus predisposed the tissue to break up into thin laminæ along the lines intervening between these layers, as well as along the fewer lines indicated by the thickened peripheral margin of each primary disk.

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* In pointing out the fact that the Balsam fir (*Abies balsamea*) has a discoid pith, Dr. DAWSON says:—"This modern *Sternbergia* is not produced by the mere breaking of the cellular tissue transversely by elongation of the fibre, but, as I pointed out many years ago in the case of the Coal-formation *Sternbergiæ* ('Canadian Naturalist and Geologist,' 1857), is a true organic partitioning of the pith by diaphragms of denser cells opposite the nodes, as in *Cecropia peltata* and some species of *Ficus*. The transverse diaphragms are composed of denser cells flattened horizontally, and they are, as in *Sternbergia*, accompanied by constrictions of the medullary cylinder."—*Nature*, May 15, 1873.

In my *Sternbergia* described above I find no evidence of two kinds of cells; but there is an unmistakable horizontal stratification of them which obviously approximates closely to Dr. DAWSON's description.

Fig. 46 is another transverse section from the same locality as the last, enlarged $4\frac{1}{2}$ diameters. It is peculiar inasmuch as it is one of the very few examples of a *Dadoxylon* in which I have seen concentric lines of growth in the woody zone. Two or three of these lines are sufficiently distinct. The more peripheral ones are more faintly marked.

There yet remains to be considered some peculiar divergent elements which exist in several of the specimens. These are best seen in the transverse sections figs. 44 & 46. In fig. 44, *g*, *g*, two narrow vascular bundles arise from near the innermost margin of the woody cylinder, and move in nearly parallel lines towards the periphery. Fig. 47 represents a portion of the vertical section fig. 45, enlarged fifteen times, but from a part not represented in the latter figure; we here see, at *g*, one of the bundles corresponding to those of fig. 44, *g*, *g*, intersected almost, though not quite, radially. We here discover that the vessels *b'*, bending outwards to form the bundle *g*, are derived from the innermost layer of the woody cylinder *b*. This orientation of these small bundles from the innermost layers of the wood is suggestive of their primary relation to leaves rather than to branches. A similar bundle departs from the interior of fig. 43 near *g*. Fig. 46 exhibits two of these pairs of bundles at *g*, *g*. This example is valuable, because it demonstrates, through their angle of divergence, that these bundles do not proceed to distichous peripheral appendages. Fig. 48 is part of a tangential section of the specimen fig. 44, enlarged 15 diameters. It has intersected two of these bundles at right angles to their direction, and is interesting as demonstrating their perfect twin-like character. The elementary tissues in the two bundles of this section are destroyed by mineralization, but the other examples clearly show that, when preserved, they consist of the ordinary discigerous fibres of the woody zone.

Fig. 49 is part of a tangential section of the ligneous zone of the Coalbrookdale branch, figs. 33–40, enlarged 62 times. The section was made close to the medulla. In fig. 38 we see a number of cells, *f*, identical with those of the medullary rays, and which, like the latter, doubtless constitute outward prolongations of the medulla. The fibres which are in contact with these cells, whether coming from above or from below, have their regular arrangement disturbed, so as to be cut through by the section; they become suddenly deflected, to make way for the cellular rays pursuing their horizontal course towards the periphery.

In the specimen, fig. 42, I find a solitary bundle given off from the inner margin of the ligneous zone at *g*; and in the vertical section of the same example, fig. 43, there is near *g* a bundle, the position and course of which correspond exactly with that seen in fig. 47. I find no traces of these bundles near the outer margins of such sections of large branches as I possess[*].

The question arises, what is the destination of these bundles? On making a series of sections of the large-leaved *Araucaria imbricata* and of *Taxus*, I find in their young branches and twigs an abundance of precisely similar bundles; but in these plants they are single, not in pairs. They are unmistakably the leaf-bundles of the *Araucariæ*; and I have no doubt that such is also the character of those in the fossil branches. Their greater number in the small branches than in the larger and more fully grown ones, their invariable origin from the innermost surface of the ligneous cylinder, and their apparent disappearance in the peripheral portions of the larger branches, are all facts which point in the same direction. But why are these bundles arranged symmetrically in pairs? Either two bundles went to one leaf with a double midrib, or the leaves were arranged in pairs, either of which conditions may prove a possible means of identifying these stems with their foliage, not to be lost sight of by those who are working amongst the plant-impressions of the Coal-measures.

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[*] This bundle is not distinctly shown in the figure.

Fig. 50 is part of a radial section of a fragment of the ligneous cylinder of a *Dadoxylon* from Binns Cliff, in Burntisland, Fifeshire, for which I am indebted to my friend Mr. Grieve. Fig. 51 is the lateral surface of one of its fibres. Its structure differs in no material respect from those already described. The disks are sometimes in two rows, sometimes in three, as represented in fig. 51. Though coming from so much lower a geological horizon than those described in the preceding pages, I can detect no material difference between it and the *Dadoxylons* from the beds above the Millstone Grit.

Fig. 52 is a portion of another fibre from a fragment from near Oldham. In it each of the disks shows a vertical central line, partly crossed by an oblique one coming from above downwards, but which becomes very indistinct after it has reached the more distinct vertical one. This is the only example I have met with from the Oldham deposits in which a trace of the central spot so universal in the bordered disks of living Conifers is to be seen.

These facts appear to me to justify the conclusion that the specimens described really belonged to Gymnospermous plants allied to the Coniferæ. On comparing the sections of the Coalbrookdale specimen (figs. 33 & 36) with several recent forms we see at a glance how close is the resemblance between them. Thus, on examining a longitudinal section of a young branch of *Taxus*, we obtain an exact copy of fig. 36, the portion *d* being the liber, with its vasa propria and longitudinal resin-canals, whilst the parenchyma (*e*) is a phelloderm layer, the cells of which, in the recent stem, are full of chlorophyl grains. Young shoots of *Araucaria braziliensis* present similar resemblances. The bark of my fossil exhibits no development of the phellem layer seen in *Salisburia adiantifolia* and similar stems. The other tissues equally display characteristic resemblances. Thus the medulla is encased in a medullary sheath (fig. 35, b'), and this in turn is invested by a xylem layer composed of thick-walled prosenchyma, without any admixture of true vascular elements. Added to this we have the orientation of the leaf-bundles; and, finally, the fine fragment, in my cabinet, figured in my memoir in the 'Monthly Microscopic Journal' for Aug. 1869, plate xx. fig. 7, exhibits a thick branch passing straight through the wood of the stem, the layers of which latter surrounding the "knot" are thrown into undulating folds, exactly as would be the case with a piece of pine-wood that had been split by means of a wedge in a plane at right angles to the direction of the imbedded branch. Hence we have every reason for continuing to class *Dadoxylon* amongst the Gymnospermæ. I refrain from attempting to assign specific names to any of these fragments, being unable to discover any distinctive features whereby to characterize them.

The larger branches found in the Ganister-beds, associated with Goniatites, rarely display any trace of bark, and what little does exist is much disorganized—an additional indication that they have been drifted from a distance and long exposed to the action of water.

Seeds.—Most of the writers who have illustrated the Flora of the Coal-measures have described some form or other of what they supposed to be seeds; but nothing was known of the internal organization of any of these objects until Dr. HOOKER and Mr. BINNEY published their memoir on *Trigonocarpon*[*]. In this memoir the *Trigonocarpon* described, which appears to have been the *T. olivæformæ* of LINDLEY and HUTTON[†], is shown to bear a close resemblance to the true drupaceous seeds of *Salisburia adiantifolia.* Though no further light was thrown upon their internal organization, numerous additional forms of Carboniferous seeds were described and figured, especially from the Coal-formations of the New World, by Mr. NEWBERRY[‡]. This scanty knowledge was suddenly increased through the discovery of a thin bed of silicified rock at St. Étienne, one of the many results of the important researches of M. GRAND'EURY. This deposit abounds in fossil seeds, many of them being of considerable size and having much of their structure exquisitely preserved. Twenty-four species of these, distributed through seventeen genera, have been recently described by M. BRONGNIART[§]. In this important memoir, which was read to the Academy of Sciences in August 1874, the veteran author merely gives small outlined sketches of the various seeds; but it was his intention to make them the subjects of a separate volume, in which the minuter details of their organization would have been elaborately illustrated. Unfortunately his recent death has robbed the Geological world of what would have been one of the most important contributions yet made to Carboniferous literature.

M. BRONGNIART unhesitatingly determined that his specimens are true seeds referable to the Gymnospermous group of plants, though he discovers in them numerous secondary modifications which are not seen in matured recent Gymnospermous seeds. His remarks on one of these modifications are so important that I quote them here:—

"Dans plusieurs de ces graines, cette extrémité (supérieure) du nucelle, que j'ai désignée dans d'anciens travaux sous le nom de *mamelon d'imprégnation*, a la forme d'un cône terminé par une sorte de bouton papilleux, et se montre ainsi avec l'aspect qu'il a dans beaucoup de graines lorsqu'on cherche le tissu mort et sphacélé de ce mamelon dans la graine mûre. Mais, dans plusieurs de ces graines, on peut même dire dans la majorité d'entre elles, et particulièrement chez celles qui s'éloignent le plus par leurs formes extérieures des graines des Conifères et des Cycadées, ce mamelon du nucelle présente une structure toute particulière, dont on n'a pas signalé d'exemple parmi les végétaux vivants. Le sommet du nucelle offre une cavité qui paraît circonscrite par un tissu cellulaire, taché et très-délicat, dont la disposition et la structure ne pourraient être bien comprises que par des figures exactes. Cet espace vide paraît s'ouvrir supérieurement au-dessous du micropile du testa. Cette communication est quelquefois bien distincte, mais souvent elle est masquée par le rapprochement des bords supérieurs de cette cavité, qui, au contraire, est largement ouverte du côté qui correspond à la partie supérieure du sac périspermique, dans laquelle devrait se trouver l'embryon. Dans un assez grand nombre de cas, on voit dans cet espace vide des graines elliptiques entourées d'une membrane bien définie, ordinairement assez colorée, quelquefois marquée d'un réseau régulier, qu'il est difficile de ne pas considérer comme des grains de pollen ayant pénétré par le micropile jusque dans cette excavation du nucelle au moment de la fécondation.

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[*] "On the Structure of certain Limestone Nodules enclosed in seams of Bituminous Coal, with a Description of some *Trigonocarpons* contained in them." By JOSEPH DALTON HOOKER, M.D., and EDWARD WILLIAM BINNEY, Esq. Phil. Trans. vol. cxlv. part i. p. 149, 1855.

[†] 'Fossil Flora,' iii. t. 222. figs. 1, 3.

[‡] 'Report of the Geological Survey of Ohio,' vol. i. part ii. Palæontology. "Descriptions of Fossil Plants," by J. S. NEWBERRY.

[§] "Études sur les Graines fossiles trouvées à l'état silicifié dans le Terrain Houiller de Saint Étienne," par M. AD. BRONGNIART. 'Annales des Sciences Naturelles,' Botanique, 5ᵉ série, tome xx.

"Je suis, en effet, porté à penser que dans la jeunesse de la graine, lorsqu'elle était encore à l'état d'ovule, cette cavité du nucelle ne formait qu'une dépression, une sorte de cupule, dont les bords se sont ensuite rapprochés comme cela à lieu pour le testa lui-même, dont la large ouverture de la primine forme plus tard le micropile. Ce rapprochement des bords de la cupule nucellaire formerait ainsi une sorte d'endostome, qui différait seulement de l'endostome ordinaire, résultant du rapprochement des bords de la secondine, en ce qu'il serait formé par les bords du sommet du nucelle lui-même."

The peculiar structure here described is found developed in a remarkable manner in several of the seeds which I have obtained from the Coal-measures of Lancashire and of Burntisland. At the same time there is one remarkable difference between my examples and the French ones. Most of the latter are large and conspicuous objects. With the exception of *Trigonocarpon* mine are of small size. Most of them also exhibit features so peculiar to themselves that, with one or two exceptions, I am unable to locate them even in the genera established by M. BRONGNIART. That author throws his forms into two groups, in one of which the seeds are more or less flattened and bicarinated, whilst in the other the transverse section is either a circular one, or exhibits three, six, or eight radiating elements symmetrically disposed around a common axis. So far as this method of grouping goes, it is applicable to my own examples; but since one of my circular forms exhibits the peculiar structure described in the words of M. BRONGNIART which I have just quoted, in the most remarkable manner, I will reverse his order of arrangement, and deal first with the seeds belonging to his second division.

The first seed which I will describe is one to which I propose to assign the generic name of *Lagenostoma*. Though it is rare in our Lancashire nodules, yet it is the one which I have found most frequently, with the exception of the *Trigonocarpon*. Owing to their small size I found it impossible to make a linear series of sections of each seed; and yet nothing could be more misleading than any isolated section is, whether longitudinal or transverse. Hence I was led to adopt what I have found to be a most useful method of examination. I ground down each seed slowly and with the utmost caution, and then carefully sketched the various characteristic appearances as they presented themselves during the progress of the grinding process. The adoption of this plan rendered it almost impossible for any important feature to escape my eye. At the same time the more important of these appearances are permanently illustrated in my cabinet by the adoption of another method which is rendered possible by the peculiar mineralization of these seeds. On reaching any structure for which it seemed desirable to retain an available voucher, I affixed the surface with balsam to a microscopic glass slide in the usual manner. I then ground away the opposite side of the seed until I arrived at a repetition of the appearances on the presentation of which my first grinding had been arrested. Having thus obtained a second verification of these appearances I advanced slowly until I reached the centre of the seed, which I protected in like manner by a covering of balsam and thin glass. Thus many of my specimens exhibit *two* sections, a central and a tangential one, both sections being examined as opaque objects by means of reflected light. The various tissues being generally dark-coloured and carbonaceous, whilst their cavities have been filled with white, infiltrated carbonate of lime, nothing can be more beautifully distinct than many of these specimens are when thus examined. Most of the specimens have been of my own finding; but I am indebted to Mr. BUTTERWORTH for an example of a *Trigonocarpon* and a *Lagenostoma*, and also to Mr. NIELD for a specimen of each of these genera, which specimens these indefatigable auxiliaries kindly allowed me to grind away, sacrificing their treasures in the interests of science.

In its perfect state the seed under consideration has the form of a nutmeg, being so broadly ovate as to approach the figure of a sphere. Its length is about ·16 and its breadth about ·1. At its upper extremity there appears to have been a slight central prominence, in the middle of which was the micropyle.

The first series of figures of *Lagenostoma* to which I would call attention represent successive sections made from Mr. BUTTERWORTH'S specimen. Fig. 53 is a rather obliquely transverse one made near the extreme micropylar end of the seed. *a* is a part of the dense testa; *d* is the uppermost extremity and central micropilar orifice of the peculiar flask-shaped cavity which I propose to designate the *lagenostome*; at *a'* we have the opposite half of the testa detached from *a* by pressure, and also intersected lower down in the seed than at *a*. We have here some crenulated outlines, *e*, becoming separated from the testa. In fig. 54, which is a section made yet a little lower down in the seed than 53, we find that the testa (*a*) has its internal surface regularly crenulated, whilst the lagenostome (*c*) appears as a small mass of parenchymatous cells, enclosed in a firm and well-defined membrane. The crenulated arches at *e*, springing from the portion of the testa at *a'*, have now increased in size. Fig. 55 is the third of this descending series of sections. We now see that two of the crenulations of the testa, at *e*, *e*, are assuming the form seen in fig. 53, *e*, whilst those at *e'* have now wholly detached themselves from the testa, *a'*, with which they were connected in figs. 53 & 54. The fourth section (fig. 56) now shows the testa *a* as a strong well-defined membrane of uniform thickness, whilst the crenulated outlines, *e*, of fig. 55 are now yet more entirely separated from the testa.

The value of this unique, though crushed, fragment lies in the clear proof which it affords us that the remarkable structure *e*, *e'*, which I propose to designate the *canopy* of the *lagenostome*, originates superiorly in the hard testa of the seed. Fig. 57, which represents a portion of the testa *a*, with the crenulations of the canopy *e* intermediate between the sections figs. 55 & 56, further demonstrates the origin of some other curious structures seen in sections to be described. At *a'* a cross bar of the testa unites that structure with the centre of each of the crenulated curves of the canopy *e*. In the other crenulations this bar is imperfect, only projecting from the testa like a tooth and not reaching the concavity of each crenulation. It is now, in fact, a small projecting ridge running down the inner surface of the testa, but which has disappeared before reaching the section 56, *a*. Figs. 58, 59, 60, & 61 represent a series of four vertical sections of another specimen made in parallel planes. Fig. 58 is made nearly midway between the most prominent lateral surface of the seed and its centre. Fig. 59 is made midway between fig. 58 and fig. 60, which latter has been carried exactly through the central axis of the seed; whilst fig. 61 is from a plane nearly corresponding with fig. 59, but on the opposite side of the seed to the latter section. Fig. 58 exhibits the testa *a*, which appears to have been a hard, dense, sharply defined structure, having a thickness of from ·01 to ·005, but usually having a mean thickness of ·006. I have not been able accurately to determine its composition, but there are indications that it consists of an assemblage of oblong cells. Immediately within it is a second and much thinner membrane *f*, but which obviously has adhered closely to the inner surface of the testa, as is shown by the numerous irregular shreds of tissue which still link the two together. I have designated this the *nucular* membrane. This term seems conveniently to represent M. BRONGNIART's term "membrane nucellaire." Within this again is the third membrane *g*, which is also extremely thin. This I regard as the membrane immediately investing the endosperm, the "membrane périspermique" of M. BRONGNIART.

In fig. 59 all these tissues reappear at the lower part of the seed, as in fig. 58; but at its upper part we find new conditions. The endospermic membrane *g* is there removed much further from the testa than at its opposite end. The nucular membrane *f* splits into two portions at *f'*, *f'*. The outer layer, *e*, *e*, ascends towards the upper part of the testa, where it is inflected into four or five crenate curves, the concavities of which are directed towards the inner surface of the testa, the outermost ones on each side being the largest. Within this structure, which I have designated the *canopy*, we find another space partly filled by a very delicate parenchyma, *h*, and enclosed superiorly by the innermost, *c*, of the two layers into which the nucellar membrane splits at *f'*, *f'*.

In fig. 60 definite relationships become assignable to each of these tissues; *d* clearly represents the position of the micropile, immediately below which we have the flask-shaped cavity, *c'*, which I have designated the lagenostome, the *cavité pollinique* of M. BRONGNIART. This cavity is bounded by a thin but very well-defined membrane, *c*. Its length, from the surface of the testa at the micropile *d* to the base where its walls appear to converge, is about ·04. In its interior is a mass of very delicate and regular parenchyma, *h*, the densest portion of which extends upwards into the neck of the lagenostome towards the micropile, but which neither here nor yet lower down entirely fills the cavity of the lagenostome. The single nucular membrane *f* subdivides at its upper portion into three parts; one of these, *f'*, is deflected on each side so as to become united with the upper part of the endospermic membrane at *g'*. The second one, *f''*, proceeds upwards and inwards, on each side, to unite at an acute angle with the lower part of the wall of the lagenostome. A third part passes upwards, on each side, to form the canopy *e*. This latter structure appears in this specimen as if it was a membrane of considerable thickness; but such is not the case. This structure is thrown into remarkable longitudinal folds, and the section has here cut through the membrane in the superficial plane of some of these folds. Its thin sharp edge can readily be discerned in the original specimen. The endospermic membrane, *g*, is here seen to be detached some distance from the base of the lagenostome; but this will be shown not to have been the normal condition of the seed. A faint line connects the two sharp basal angles formed by the walls of this lagenostome; but this line has evidently not been a continuous tissue, but merely some minute and detached organic atoms which have been torn from some other tissue upon which the base of the lagenostome rested. So far as this section is concerned the centre of that base has been an open one.

Fig. 61 is a tangential section which has passed through the oblate sides of the lagenostome but has not intersected its narrow neck. In its leading features this section corresponds with fig. 59. Thus we have the testa, *a*, connected with the nucular membrane, *f*, by numerous shreds of tissue. That membrane divides at its upper part, on each side, into three layers, as in fig. 60, whilst at its upper part it further exhibits the crenulated contour of fig. 59; but here the crenulated curves are but three in number. The thin wall (*f''*) of the lagenostome *c'* is now seen to exhibit a clear wavy outline, whilst within the interior of the cavity we have a well-defined mass of parenchyma (*h*). This being an important section I have, in fig. 62, enlarged the upper part of it to 80 diameters. The testa (*a*) exhibits the three crenulations *a'*, *a'* which are just detaching themselves to form three of the folds of the canopy *e*. The wavy outline of the wall of the lagenostome appears at *c*. The nucular membrane has united itself to the endospermic membrane at *f*; but at *f'* traces of the separation of these two membranes present themselves. At *g* the nucular membrane seems to be firmly adherent to the delicate parenchyma (*h*) of the lagenostome, whilst that parenchyma has shrunk away from the lateral wall (*c*) of the cavity in a very regular manner, leaving an open space (*c'*) all round the cellular mass. This last point is, as we shall shortly perceive, an important one.

Figs. 63 to 68 represent a second consecutive series of sections of another example of *Lagenostoma ovoides*, of which fig. 67 is the central one. This specimen exhibits some new features. Fig. 63 is the most peripheral section. Here we see the testa (*a*) evidently deprived of some of its outer portions; hence the apparent reduction of its thickness. The specimen was almost wholly detached from its matrix before I became aware of its nature; hence part of the testa was left adherent to the stone. In this example also the perispermic membrane has either disappeared altogether, or it is in close contact with the surrounding nucular membrane *f*. At *e* the section has passed nearly in the plane of several folds of the canopy. Fig. 64, *e*, shows two of the folds of the canopy intersected longitudinally, whilst a third intermediate one has been crossed tangentially, forming the central loop in the section. In fig. 65 the central fold of the canopy (*e'*) has been divided close to the micropyle, bringing into view, at *c*, a tangential section of the wall of the dilated portion of the lagenostome. In fig. 66 we have nearly the same appearance as in fig. 65, only the section has now laid open the cavity of the lagenostome (*c*). Fig. 67 is, as already stated, the central section of the series, and since it shows some instructive features at its upper extremity I have given an enlarged sketch of this portion of it in fig. 69. Fig. 68 is also important. It is obvious that the structures *e*, *e* are the boundary walls of the canopy, whilst at *e'* we have two intermediate folds cut off near the micropyle. At *a'*, *a'* the inner portions of the testa have been detached from the outer ones, which latter have adhered to the matrix (*a*). In the centre of the seed is a dark-coloured patch which looks like an embryo-sac, but I believe it to be merely an effect of mineralization. At *i* we have an organic thickening of the nucular membrane uniting it with the testa *a*, and projecting from the same part we have the cellular mass *k*, which is either a funiculus or a small portion of the foliar or peduncular structure upon which the seed has been planted in a sessile manner; whichever was the case we have here clear proof that *Lagenostoma ovoides* was an orthotropous seed.

Fig. 69, which represents the upper part of fig. 67, enlarged 80 diameters, exhibits the testa (*a*) split into two layers at *a'*, two folds of the canopy at *b*, *b*, and the upper part of a third intermediate fold intersected tangentially at *b'*. The latter one hides the narrow upper portion of the lagenostome *c*. In this specimen we have a clear demonstration of the composition of the membrane forming the canopy. It consists of a single layer of prosenchymatous cells, and other examples show that this is also the structure of what I have called the nucular membrane, of which the canopy is but an extension. The lagenostome is again seen to be largely occupied by a mass of delicate parenchyma. Assuming that the nucular and perispermic membranes are united in this specimen, it appears that the base of the lagenostome rests directly upon the upper end of the perisperm or nucleus, and that the detachment, through contraction or other causes, of the base of the lagenostome from the perispermic membrane in fig. 60 has caused the nucular membrane to split up into the multiplied layers seen at *f'*, *f''* in that section.

Immediately below the right hand side of the base of the lagenostome in fig. 69 there are traces of an indentation (x) into the substance of what I presume has been the nucleus, and which contains some dark-coloured granules. These have most probably been derived from the lagenostome, yet the position they occupy may be merely an accidental one.

Fig. 70 represents the upper extremity of another very instructive specimen, enlarged 80 diameters. A portion of the testa remains at a. The nucular membrane (f) is in near approximation to the testa, whilst the perispermic membrane (g, g) is now so much distended as almost to occupy the entire cavity of the seed. At f', f'' we again see, as in figs. 60 & 61, the nucular membrane f, splitting into three layers. On the left hand, one of these ($f1$) goes to the upper surface of the perispermic membrane (g), the second ($f2$) goes to the base of the lagenostome (c), whilst the third and uppermost ($f3$) forms the canopy. On the right hand, the innermost layer ($f'1$) first comes into contact with the perispermic membrane, but soon leaves it again, bending upwards so as to reach the base of the lagenostome. The second ($f'2$) goes direct to the lagenostome, as on the opposite side; whilst, in like manner, the outermost layer ($f'3$) again forms the canopy, additional shreds of which are seen on both sides, yet more externally. Here, again, the membrane of the canopy appears to be thick, but it is not so. Its true thickness is represented by the dark boundary line at the inner margins of $f'3$ and $f3$; the rest is merely a continuation of one of the folds seen perspectively, as in fig. 69.

The lagenostome is here very clearly shown to be open inferiorly so as to bring the base of its internal cavity into intermediate contact with the upper part of the perispermic membrane g'. But this fine section further demonstrates that the membranous wall of the lagenostome (c, c) is composed of prosenchymatous cells like those constituting the canopy and nucular membrane, whilst it differs altogether, as I shall be able to demonstrate, from the structure of the perispermic membrane, g'.

But the most interesting feature of this section appears in the interior of the lagenostome c. Its central portion is occupied by the usual delicate parenchyma, but in the vacant space remaining between this parenchyma and the prosenchymatous wall of the lagenostome are twelve or thirteen round clearly defined bodies (l, l), which are unquestionably identical with those seen by M. Brongniart, and regarded by him as pollen grains. These little spherical bodies have a mean diameter of about ·0023, and exhibit every appearance of being true pollen grains. We shall find them again in the next specimen to be described.

Satisfactory, and in some degree conclusive as all these several features of the vertical sections proved to be, there yet remained some important unsolved problems; but my materials were exhausted. I had no satisfactory evidence respecting the horizontal arrangements of the lagenostomal region. To relieve me from this dilemma my friend Mr. Butterworth sacrificed his only specimen of this seed; unfortunately it proved to be so crushed that it failed to give me all the information I sought, though, as we have seen from figs. 53–57, it revealed what none of my other specimens had done, the horizontal arrangement of the parts near the mouth of the micropyle. One other seed remained in the cabinet of my equally valuable auxiliary Mr. NIELD, and this he, too, resigned to the grinding-wheel. Nothing could be more satisfactory or rewarding to so true a lover of science than the results of this devotion. Unfortunately Mr. NIELD had ground away a little of the extreme micropylar extremity of his specimen before he became aware of its value. When it came into my hands the ground extremity exhibited the appearance seen in fig. 71, which, enlarged 15 diameters, reveals the exquisitely symmetrical arrangements characterizing the membranes in the lagenostomal region when undisturbed by pressure, mineralization, or the shrivelling of the tissues prior to fossilization. This seed was wholly detached from the matrix, which had, as is so usual, carried away with it the greater portion of the testa. Nevertheless the outline of its inner surface *a* is perfectly preserved. Within this testa we have the crenulated outline *e*, which represents the transverse section of the canopy, consisting of ten crenulated curves, the rounded concavities of which face outwards, where they are separated from each other by sharp projecting folds of the membrane, arranged in a regularly radiating order, but the angles of which do not reach the testa. Within this fluted canopy we have the upper part of the lagenostome (*d*), also fluted in correspondence with the canopy, though the inflexions of this membrane are much less prominent than are those of the canopy itself.

Fig. 72 is a second transverse section of the same seed made across the broadest part of the lagenostome, enlarged 30 times. At *a* we have a portion of the testa. At *e* is the canopy exhibiting the regular crenulated outline already described; we here see that it presents a sharply defined margin at its inner border, whilst externally it has a torn and shredded aspect, indicating a former attachment to the inner surface of the testa *a*. This attachment is further indicated by the innumerable delicate fragments of cellular tissue which occupy the concavities of the folds of the canopy. In the centre of each of those concavities there is a dark spot (*n*). I cannot detect at these points any specialized structure beyond traces of a few dark-walled cells, and it seems extremely probable that these may have been elements left behind for some purpose by the transverse bars (a') seen in fig. 57. On the opposite side of the seed (e') the regular crenulations of the membrane forming the canopy have disappeared, probably as the result of shrivelling prior to mineralization. At *c* we have a section of the wall of the lagenostome. We have already seen from fig. 70 that it was composed of prosenchymatous cells. We now find that it consists of a single layer of such cells.

Fig. 74 represents a few of the prosenchymatous cells composing the canopy, as seen in the specimen fig. 70. In fig. 72, *c*, these cells are intersected transversely; at *c*, c' we see indications of the regular crenulations which originally characterized this lagenostomal membrane, but which have here in a great measure disappeared, through shrivelling; mineralization also has somewhat modified its normal aspect at c''.

Near the centre of the lagenostomal cavity we have a small portion of the parenchymatous tissue (*h*) seen in the same position in so many of the other sections, only here it is reduced to an exceedingly small amount. Scattered throughout the open space surrounding this parenchyma are numerous small rounded bodies (*l*), apparently identical with those seen in fig. 70. These are the supposed pollen grains.

Fig. 73 is another section of the same seed, made parallel to fig. 72, only nearer the base of the lagenostome. The tissues reappear as in the last section, except that the upper half of the wall of the lagenostome, *c*, exhibits the crenulated figure of that membrane in a more regular manner than before, and at *g* we now see the uppermost surface of a portion of the perispermic membrane. The appearances presented here are extremely significant. This membrane is altogether different from those already described; a small portion of it, enlarged 175 times, is seen in fig. 75. It is apparently structureless, except that it is full of minute translucent spaces usually exhibiting sharply defined angular contours, and which look as if the membrane had been filled with minute crystals[*], which I believe to have been the case, unless these objects were crystalloids, which is not improbable. It is perfectly obvious from this section that the prosenchymatous wall of the lagenostome did not extend across the base of the latter structure, but that, in its normal state, the cavity of that lagenostome was in direct contact with the membrane of the perisperm, and that consequently nothing intervened between the pollen grains in the lagenostome and the perispermic cells but this thin membrane. Thus it follows that in this seed the lagenostome (the "cavité pollinique" of M. BRONGNIART) has been formed by a separation of the nucular membrane from the perispermic one at the upper extremity of the perisperm.

I have entered upon the minute details of the preceding description because I believe this remarkable seed to be essentially a typical one. It appears to be much more elaborate in its organization than most of those described by M. BRONGNIART, yet it obviously belongs to the same class of seeds as those discovered by M. GRAND'EURY.

That no known recent seed corresponds exactly with these has already been pointed out by M. BRONGNIART. But that distinguished botanist read a brief memoir to the Academy of Sciences in Paris in September, 1875[†], in which he indicated the probability, from observations which he had made upon the fertilized ovules of some recent Cycads, that *in their young states* the seeds of this class of living plants exhibit conditions not dissimilar from those found in the Carboniferous fossils; but the notice published in the 'Comptes Rendus' is too brief to contain the details needful for a thorough settlement of the exact degree of identity. On the other hand it appears to me very important that the early development of the fruit of the *Salisburia* should be studied in reference to this question. Dr. HOOKER long ago pointed out the many features in which this Chinese fruit resembled the fossil Trigonocarpons; but there is one peculiarity in it which he has not noticed, but which appears to bear upon the present inquiry. On opening the hard endotesta (fig. 76, *a*) of one of the dried seeds we discover immediately within that structure, and in close contact with it, the firm membrane (fig. 76, *f*) described by Dr. HOOKER. In its lower or chalazal half this membrane is much more dense than at the opposite end of the seed. In the former part it consists of a thin, almost structureless membrane, thickened by the addition of the numerous reticulated cells, already described by Dr. HOOKER, and derived from the innermost portion of the hard endotesta; but near the middle of the seed, not only do these cells disappear from the membrane, but the latter now splits into two layers—an inner one (f') which closely invests the perisperm, and an outer one (f'') which lines the endotesta, the subdivided membranes thus leaving a dome-shaped cavity between them. Fig. 76 is a diagram representing this arrangement, but exaggerating the distances beween the several tissues[*]. At all events the position of the cavity corresponds closely with that of my lagenostome, whether viewed in its relation to the nucleus or to its investing membranes.

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[*] This appearance is almost identical with that presented by the surfaces of the spicular sclerogen cells observed by Dr. HOOKER in the parenchyma of the stock of *Welwitschia mirabilis* (Trans. Linnean Society, vol. xxiv. pl. xii. figs. 5, 6).

[†] Comptes Rendus, tome lxxxi. no. 47.

When making a few observations on these seeds at the Bristol Meeting of the British Association in 1875, I proposed for a second seed the generic name of *Physostoma*; but since my only specimen of it does not exhibit its outer layer it may be better to unite it generically with the last species under the name of *Lagenostoma physoides*.

Having found but a single example of this very distinct seed I have only been able to study it under the two aspects represented in figs. 77 & 78. The former of these is a longitudinal section made nearly through the centre of the seed, and the latter is one made in the same vertical plane, but nearer to the periphery of the seed.

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[*] Throughout this memoir I have employed the two terms *perispermic* and *nucular* to designate the two membranes usually found within the testa of these seeds. I have done so for the purpose of making my nomenclature harmonize with that of the late M. BRONGNIART's memoir on similar Carboniferous seeds from St. Étienne. At the same time I may point out not only the extreme difficulty of identifying these membranes with those of the seeds of living Cycads and Conifers, but also the objections which British botanists may reasonably entertain to the nomenclature employed. On the former point we must remember that during the long protracted development of the ovules of the varied members of the Cycadean and Coniferous families the equivalents of the spermoderm and embryo-sac undergo very important changes, rendering the exact comparison of the recent and fossil forms very difficult. The second difficulty referred to is equally important. British botanists apply the two terms perisperm and endosperm very definitely: viz. the former to the tissues of the nucleus, and the latter to the contents of the embryo-sac. The nomenclature of M. BRONGNIART has not recognized this distinction. He speaks of the "sac périspermique, dans laquelle devrait se trouver l'embryon." "Malgré l'altération des ces parties intérieures, on peut y reconnaître presque toujours deux enveloppes membraneuses; l'une, plus externe, naît au pourtour du chalaze ou sur sa surface supérieure, et se termine supérieurement par une extrémité conique qui correspond à l'orifice du micropyle du testa, mais qui en est souvent assez éloignée: c'est la surface du nucelle; l'autre, beaucoup plus altérée, libre et flottante au-dessus de la chalaze et se terminant à quelque distance au-dessous de l'extrémité conique de la précédente, correspond à l'enveloppe du périsperme."

These definitions make it clear that the "perisperme" of M. BRONGNIART is the endosperm of British botanists, whilst his "membrane périspermique" is the true primary embryo-sac. His "membrane nucellaire," on the other hand, is the spermoderm of other writers. With this explanation of the sense in which I employ M. BRONGNIART's terms, I can continue to use them in order to avoid confusion on comparing his descriptions of the St.-Étienne seeds with mine. At the same time a different use of these terms would have been preferable. —July 19, 1877.

As already observed, the outermost tissues of this seed are missing, and those of its micropylar extremity are also somewhat deranged. Its present entire length is about one sixth of an inch. Its nucleus (*n*) has a total length of ·09 and a maximum diameter of ·04, but a mamilliform projection exists at its micropylar extremity (n'), which is rather more than ·02 in length and rather less than ·02 in diameter. The nucleus is invested by a thick layer of prosenchymatous tissue (*b*), of which the cells are of great length; whether this tissue is an endotesta, or an extremely thick nucular membrane closely united to a perispermic one at its inner surface, I am unable to say; but I am inclined to adopt the latter explanation. Above the nucleus is a curious lagenostome, figs. 77, *c*, & 78, *c*. In fig. 77, which intersects the lagenostome through its centre, we see that this organ overlaps the mamillated extremity of the nucleus on every side, as if it were a soft bladder half full of water, allowed to rest upon and overhang the neck of one of the old-fashioned soda-water bottles. The upper extremity is imperfect in fig. 77, but it has apparently been prolonged upwards towards the micropyle *d*. The structure of this lagenostomal part of the seed is further illustrated by fig. 79, which represents another section of the lagenostome made so very close to fig. 77 as virtually to represent the same elements. The dark line e', e' is evidently the innermost portion of the tissue *b*, *b*. We see that this line passes downwards on each side of the nucleus to e'', e'', where it becomes deflected upwards, for a short distance close to the nucleus, *n*, after which it first bends rapidly outwards and then gradually inwards, forming the outer wall (c') of the lagenostome *c*. At the upper part, especially where this membrane contracts to form the narrow neck of the lagenostome, we discover a cellular structure in its interior. In fig. 77 we see that the two lines e', e' are prolonged upwards in connexion with two masses of prosenchymatous tissue *e*, *e*, which latter I doubt not form several of the folds of a crenated canopy. Their aspect in fig. 78, *c*, *c*, *c* tends to confirm this conclusion, because we here have clearly shown their crenated outline. The circumstance that the prosenchymatous tissue, *b*, extends upwards in all the figures, to constitute the tissues of this canopy, seems to indicate that the whole of this structure is merely a thick nucular membrane, even the endotesta of the seeds being entirely absent. In fig. 77, c', we have the outer wall of the lagenostome; but this section being virtually a tangential one, it has intersected that organ externally to the canal of the micropyle; hence it appears here as a closed cavity.

At fig. 79, which is an enlarged representation of the upper part of fig. 77, we have a mass of delicate parenchyma, seen on both sides of the seed, adhering to the inner wall of the lagenostome. This tissue has apparently extended as an interrupted ring round the interior of that organ. It is not seen in fig. 78; but it may have been deficient in the part of the lagenostome intersected in that section. I think there can be little doubt but that this parenchymatous tissue is identical with what I found in the lagenostome of *L. ovoides*, figs. 60 & 61, *h*. The nucleus *n* appears to be surrounded by an unusually thick perispermic membrane (*g*), which is continued round its mamillated extremity; but since this membrane merely appears as a dark outline, I cannot ascertain its structure; it seems to merge in the prosenchymatous tissue (*b*) with which it is invested. It is possible that this latter tissue may have formed the true endotesta. I have already shown that the nucular membrane of *Lagenostoma ovoides* appears as if little more than the innermost layer of the true testa, with which it is often more or less united by torn fibrous threads. Hence it is not improbable that in the seed under consideration all these structures, including the perispermic membrane, may have remained permanently adherent to each other, excepting near the apex of the seed, where the formation of the lagenostome involved their partial separation. This seed is also from the Oldham nodules.

The next examples belong to a somewhat different modification of this Carboniferous type; and since in both the two forms of it which I have found, what appears to represent the supposed lagenostome has a curious funnel-shape, I have assigned to them the provisional name of *Conostoma*. Figs. 80, 80*, & 81 are three sections of a distinct species from the Oldham nodules for which I propose the name of *Conostoma oblonga*. Fig. 80 is a longitudinal section made nearly through the centre of the seed, enlarged 15 diameters. The testa (*a*) is here thin throughout the greater part of the seed, but thickened at its apex. Fig. 80 *a* represents a longitudinal section of this latter part, broken away from the end (*p*) of fig. 80, and enlarged 35 diameters. Before I made a section of this fragment its inner or lower surface displayed a small central projection which fitted into the upper end of fig. 79. It is composed of numerous long narrow cells or fibres, arranged in parallel lines more or less perpendicularly to the surface of the seed at its apex, but which, at its sides, are curved, so that, at their inner extremities, the fibres verge towards parallelism with the surface of the nucleus. Some of these long, narrow cells have a diameter of ·001, others of ·0007. Each apparent fibre usually consists of more than one cell arranged in each continuous line. Their transverse subdivisions are most frequently square, at others they are oblique. Their internal cavities are filled with dark carbonaceous matter, hence the dark lines giving to the testa its fibrous aspect. The regularity of their arrangement, combined with the equally regular external outline of the seed, indicates that this testa has been hard and dry. I have not detected the micropyle in fig. 80 *a*, though I searched for it. It has probably been a minute one, and hence, having escaped my observation, the section has passed on one side of it. Within the testa, and running parallel with it, is a very thin membrane (*f*), which is apparently the nucular membrane. On reaching the level of the apex of the nucleus (*n*) this membrane splits into two layers. The outer one (f'') curves upwards and then inwards and downwards on each side, terminating in a little mass (*p*) of what appears to be cellular parenchyma, and which I presume is a dwarfed modification of a lagenostome. The inner layer (f') of the nuclear membrane proceeds horizontally inwards from each side, and then curves upwards so as to form a dome-shaped arch, the keystone of which is again the cellular mass (*p*). The breaks in its continuity in this specimen are obviously accidental ones. Within this nucular membrane and completely detached from it is the perispermic membrane (*g*), investing the nucleus (*n*). I have not been able to satisfy myself whether the lower layer of the nucular membrane (f') passes under the cellular mass (p) or whether it merely becomes merged in its cells; but I think that the former is its real condition.

Fig. 80* is a second longitudinal section made a little on one side of the last. To the left of the figure the nucular membrane (f) is closely united to the inner surface of the testa (a). Above the nucleus the nucular membrane (f) still subdivides into the two layers (f', f''), the former of which still forms a dome (c), whilst the latter is in contact with the testa. But the little point of parenchyma (fig. 80, p) is no longer seen, the section having passed on the outer side of it. In the interior of the nucleus (n) is some carbonaceous matter which appears as if composed of remnants of the perispermic cells.

Fig. 81 is a yet more lateral or tangential section which corresponds with the last, except that having passed on the outer side of the dome-like space (fig. 80*, c) it has intersected the shelf-like extension of the nucular membrane (f'), which here stretches across the seed in a straight line, whilst the upper part (f'') is in the closest connexion with the testa a'.

Fig. 86 represents a vertical section of what I believe to be a second example of this species. It has lost some parts of its testa (a), but on the other hand it adds somewhat to our knowledge of the arrangement of the membranes near the apex of the seed. The nucleus (n) is large, and is full of minute dark points, which at the first sight resemble microspores, but which I believe are merely remnants of the cells of the perisperm. At one point near the centre of the seed a mass of these cells is aggregated into a dark-coloured mass. Towards the lower part of the seed we only see traces of one membrane (f) investing the nucleus, and which I presume consists of the two membranes (f & g) of fig. 80 united into one. Above the apex of the nucleus we find this membrane splitting, like the membrane f in fig. 80, into the two portions f' and f'', which follow the same course as in the latter figure. But the small mass of cellular parenchyma (p) is now larger than it is in fig. 80, besides which we here find on each side of the seed a third membrane $f''' f'''$, which has evidently followed the curvatures of the inner surface of the testa, and though now slightly detached to the left, its apex has evidently fitted into the base (d) of what has evidently been the micropyle. These conditions clearly demonstrate, what my own specimen failed to do, that the parenchyma (p) occupies the interior of a lagenostomal cavity. In all probability the peculiar cavity (c) was originally occupied by a mamillated prolongation of the nucleus. Fig. 86 clearly demonstrates that the nucleus entirely filled the cavity enclosed within the membrane f of fig. 80, so far as the sides and base of the seed were concerned, and most probably also at its apex. If so, the lagenostomal parenchyma (p) would rest, as in *Lagenostoma ovoides*, directly upon the apex of the nucleus. The mean length of *Conostoma oblonga* has been about ·18 to ·2.

The rich deposit at Burntisland has hitherto only furnished me with good examples of one seed, which appears to be of the same generic type as the last; I propose for it the name of *Conostoma ovalis*. Fig. 82 is a vertical section through the centre of one of this species. In it the membranes are much disturbed, still they are not difficult of identification. There is an external testa (*a*) of uniform thickness. At the lower part of the seed we find an outer membrane at *f*, separating from an inner one at *g*. I presume that the former is the nucular membrane, and the latter the perispermic one. The perisperm, *n*, has obviously been much shrivelled before mineralization. The upper or apical part of the seed is so important that I have given an enlarged view of it in fig. 83, magnified 60 diameters. The testa (*a*) surrounds this end of the seed. At *f* we find an inner membrane separated from the testa. Fig. 82 shows that this is the nucular membrane, if not that and the perispermic membrane, *g*, united into one. At *f'f'* a portion of this membrane turns horizontally inwards, as in figs. 79 & 80, whilst, in a similar manner, another portion of it (*f'f'*) arches round the apex of the seed in close contact with the inner surface of the testa. The parts forming the representative of the lagenostome perplexed me until I discovered the specimen (fig. 84), which is an oblique section passing along a plane corresponding with the line *x* in fig. 83. In fig. 84 we have the testa at *a*, but apparently splitting into two layers at the upper part of the figure (*a'*, *a'*). At *ff* is a distorted membrane, of which the portions *f'f'* obviously correspond to the parts indicated by the same letters in fig. 83. Above this the membrane rises into a dome-shaped structure at *c'*, and is now very distinctly seen to consist of prosenchymatous cells. As these cells approach the central axis of the seed, they obviously bend inwards and downwards, forming a central funnel-shaped depression leading to the representative of the lagenostome (*c*). It thus appears that the part represented by fig. 83 (*c'*) is in its normal position, whilst the corresponding rounded lip of the funnel on the opposite side (*c''*) has been accidentally lifted up from its true position. At fig. 83, *c'''*, we have the semblance of a horizontal continuation of the tissues of *c'*, *c''* uniting the extremities of these two curved structures; but I do not believe that this is actually the case. We see a similar condensed tissue occupying the same position in fig. 84, but without exhibiting any trace of the conspicuous prosenchymatous structure characteristic of the wall (*c'*) of the lagenostome. Within the lagenostomal cavity we have both in figs. 83 & 84 a mass of delicate, thin-walled parenchyma (*c*), which I think must be identical with that seen in the lagenostome of *Lagenostoma ovoides*, and which, occupying, as it does in the seed under consideration, a central position, apparently closing up the entrance from the micropyle into the lagenostome (*c*), throws some light upon the similarly placed tissue (*p*) in fig. 79. At *f''* in fig. 84 we have a layer of membrane which appears to be identical with the layer *f''* in fig. 83. The length of this seed is about ·1, and its diameter about ·05 to ·066. Fig. 87 is a longitudinal section of a seed from Burntisland, which may possibly be a state of *Conostoma ovalis*, but which I am inclined to regard as distinct. It is longer and narrower than that species, besides which I cannot identify the tissues in the two forms. In its general contour it resembles *Conostoma oblonga*. There is a thin testa at *a*, within which are other membranes difficult of identification. Thus at *b'* we have one thickish membrane; at *b'' b''* this membrane splits into two. At the apex of the seed we have an outer membrane at *b*, which appears to be a continuation of *b''*. At the upper part of the seed we have an arrangement which appears to correspond approximately with that of fig. 79. Thus the membrane *f* divides above the apex of the perisperm into the two layers *f'* & *f''*. At first I regarded *f'* as the perispermic membrane; but the transverse portion *f'* distinctly exhibits a coarsely cellular structure, as at *f''* we have an equally distinct prosenchymatous one,—conditions that differ from all the unmistakable perispermic membranes of other examples. I find at *h* a small mass of cellular tissue which appears to me to be part of the parenchyma of the interior of the nucleus. If this is so, it follows that the true perispermic membrane is here blended with the nucular one throughout the greater part of its course, as in fig. 86. Since the seed thus appears to occupy an intermediate position between *Conostoma oblonga* and *C. ovalis*, it may be provisionally recognized as *Conostoma intermedia*. Its length is about ·13 and its diameter ·07.

Fig. 85 represents the external aspect of a small seed obtained by Mr. AITKEN, of Bacup, from the Ganister bed near Oldham, in which such objects are very rare. I believe it will prove to be a distinct species of *Conostoma*; but unfortunately its internal structures are imperfectly preserved, but enough remains to show that it is distinct from all those which I have described.

Figs. 88–93 represent five sections of another remarkable seed which is very distinct from those already described, but of which I have only found one solitary specimen. Hence I have adopted the plan which I have followed in several other cases. I began grinding at the exterior of the seed, where I first obtained the appearances represented in fig. 89; and as I ground deeper into the seed I sketched the structures which successively presented themselves until I reached what appeared to be nearly the centre of the seed, as seen in fig. 93. I believe that the centre of the apex of the seed is approached in fig. 92, but I was nearer the centre of the chalazal extremity in fig. 93. The irregularities in the outline of this seed demonstrate that the outer layer (*a*) was a fleshy sarcotesta, capable of being irregularly shrivelled up. It consisted, as is seen in figs. 88 & 93, of parenchyma composed of very minute cells. The greater part of fig. 89 is a tangential section made in the plane of this sarcotesta, with a small projection (*k*) at its chalazal base, which, I presume, has been part of a funiculus. Near the apex the tissues of the sarcotesta open out to form a wide micropyle, terminating, in this section, in two thin and converging margins (*a' a'*)*. Within these margins there is a ring (*h*) of delicate parenchymatous tissue. The opening in its centre obviously shows that the section has here intersected a cylindrical canal. In fig. 90 we find that the section has not only reached, but partly passed through a thin and delicate prosenchymatous structure (*f*), which is apparently the nucular membrane. At the micropyle we still have the upper ring (*h*), but we now find a similar one (*h'*) at the lower end of the micropylar cavity. Fig. 91 shows still more clearly the tissues of the sarcotesta (*a*) and the nucular membrane (*f*); but we now see, in addition, a small portion of the surface of a yet more internal membrane (*g*), which I presume is the perispermic membrane. At the micropylar end we still find what may be termed the exostome, or orifice of the sarcotesta, open; but the two rings *h* and *h'* are now linked together by a continuous band of parenchyma, though their open canals are still distinct. In fig. 92 we advance still further. The section has now passed *through* the perispermic membrane (*g*), whilst the two rings *h*, *h'* are now united into a common oblong cavity, *h*. These sections make it clear that the rings of parenchyma of the other figures and the blended portions of fig. 92 form parts of a tube or canal bent into the form of a tube, which the section fig. 89 has intersected at *a* in the accompanying woodcut, fig. 90 at *b*, 91 at *c*, 92 at *d*, and 93 at *e*.

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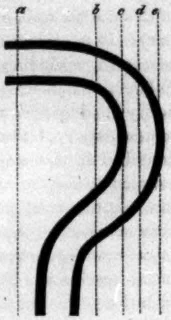
* Dr. DAWSON has figured a seed from the Devonian Beds of New Brunswick, under the name of *Cardiocarpon cornutum* ("The Fossil Plants of the Devonian and Upper Silurian Formations of Canada," Geological Survey of Canada, 1871, p. 60, pl. xix. fig. 214), in which the testa "divides into two inflexed processes at top."



Whether the flexed condition of this tube was a normal one, or whether it has been the result of partial desiccation is doubtful; but I incline to the opinion that it was originally straight. In none of the sections was I able to trace any connexion between this tube and the membranes investing the nucleus; but unfortunately I had only half the seed to work upon; had I had the other half, so as to have been able to follow up the section fig. 93 with another one, in all probability the transverse thin band of sarcocarp seen at fig. 93 *a'* would have disappeared, and the direct continuity of the micropyle from the exostome to the nuclear membrane have been demonstrated. Fig. 93 represents, as I have already pointed out, the section nearest to a central one of any of the series. At its apex, *a*, the exostome of the micropyle has disappeared. A thin extension of the sarcotesta now completely arches over the micropylar canal, from which it seems that this latter organ was somewhat curved, as well as the parenchymatous tube which it contained. In the body of the seed we see the nucular area, *n*, large and oval, nearly filling the interior of the seed, and enclosed within its perispermic membrane (*g*). External to this is the delicate prosenchymatous nucular membrane (*f*) lining the sarcotesta (*a*). But we now obtain proof that the sarcotesta was hardened at its inner border (*b*, *b*) into an endotesta, the tissues of which were much more firm than the parenchymatous elements which surrounded it. This is very distinctly seen at the chalaza (*i*), where the hardened layer of the sarcotesta is interrupted, allowing a direct communication to exist between the softer parenchyma of the sarcotesta, which is here very thick, and the prosenchymatous nucular membrane at *f'*. At this end of the seed we further see the sarcotesta prolonged into what looks like a funiculus (*k*).

Fig. 88 represents the upper part of fig. 92, enlarged 50 diameters. In this figure the differences between the three tissues of the sarcotesta (*a*, *a*), the nucular membrane (*f*, *f*), and the parenchymatous structure (*h*) within the exostome are well shown, especially that between the *parenchymatous* cells within the exostome and the *prosenchyma* of the nucular membrane, of which latter, however, the former may possibly be merely a prolongation.

Whilst I believe that I have correctly interpreted the homologies of the nucular region of this distinct seed, difficulties present themselves in reference to the micropylar extremity. It is evident that there was a very wide micropyle, so far as the sarcotesta is concerned, reminding us of what we see in the young states of *Juniperus*, *Taxus*, *Callitris*, and some species of *Pinus*; but we see, from fig. 94, that this cavity was nearly filled up with the cellular parenchyma (*h*). Is this latter tissue in any way the representative of that seen in the lagenostome of several of the species previously described in this memoir? If not, did the pollen-grains pass down the open space between it and the canal in the sarcotesta, or did they descend to reach the nucleus through the canal passing through the centre of the parenchyma? At present no satisfactory answer can be given to these questions, and I only propose them to indicate points awaiting further investigation, but for which we must await the discovery of additional specimens This seed is from the Oldham nodules. Its maximum length, exclusive of the funiculus, is ·25.

The next seed to be examined is the *Trigonocarpon*[*] *olivæforme*, which was long ago made the subject of the memoir by Dr. Hooker and Mr. Binney already referred to, published in the 'Philosophical Transactions,' vol. 145. These seeds are found in great abundance in a quarry of sandstone belonging to the Upper Coal-measures, known as the Peel Delph, near Worsley, in Lancashire. The chief accumulation of them in that quarry is in a very thin seam, in which they are clustered together in the greatest profusion, having evidently been drifted from some locality where the trees producing them overhung the water, and into which they had been shaken, when ripe, by some violent primæval tempest, and then drifted upon a neighbouring sand-beach. Other drifted plants of the Coal-measures are frequently met with in the same locality. These seeds are very rare in the calcareous nodules of the various localities near Oldham. Mr. Binney met with great numbers of them in a deposit at some other place in Lancashire, but of which he has not recorded the name, beyond saying that they were from one of the Ganister coals of the Lower Coal-measures.

My two valuable auxiliaries, Mr. J. Butterworth and Mr. Nield, have met with but very few specimens of these seeds in the Oldham deposits, which belong to the same geological horizon as that from which Mr. Binney obtained his examples. Some of these specimens my two friends have kindly placed in my hands[†].

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[*] There is much confusion in the spelling of this generic term. In his original 'Prodrome' Brongniart adopted "*Trigonocarpum*." In his later 'Tableau' he changed it to "*Trigonocarpon*;' whilst in his yet later memoir on the St.-Étienne seeds he again alters it to "*Trigonocarpus*." Hooker and Binney adopted "*Trigonocarpon*;" Dr. Dawson writes "*Trigonocarpum*;" and Professor Newberry "*Trigonocarpon*."

[†] Being anxious to make this renewed study of *Trigonocarpon* as searching as possible, I placed two of these specimens in the hands of Mr. Cuttell, the well-known lapidary, with instructions to obtain for me as many

Fig. 94 represents a lateral view of the finest specimen I have hitherto obtained from the Peel Delph. These Peel specimens usually resemble the nuts of the *Salisburia* sold in the grocer's shops in China, but which are deprived of all their outermost sarcotesta. But the Peel specimens are even something more than this. There is no doubt that fig. 94 represents a sandstone cast of the interior of the hard endotesta, with a thin film of tissue remaining upon the cast, and which, even in these sandstone specimens, retains superficial traces of the prosenchymatous structure of the endotesta, within which the cast was moulded. Such specimens do not, in any way, represent the external form of the seed, as will be shown in my description. The *entire* specimen in fig. 94 represents the exact form of the interior of the testa, the lower or tumid portion of it corresponding to the nucleus with its thin investing membranes, and the prolonged upper portion, with its three diminishing concave surfaces, of which only one appears in the figure, representing the interior of an elongated micropyle. It is important that this fact should be correctly understood, since many of even the most recent references to these seeds are rendered erroneous by a confusion between their external contours and their internal casts. Thus Dr. BALFOUR describes *T. olivæforme* as "an ovate, acuminate, three-ribbed, and striated fruit or seed" ('Palæontological Botany,' p. 63), whereas it was twelve-ribbed, and we have no evidence whatever that its surface was striated. Professor NEWBERRY figures a similar example to my fig. 94*, of which he says that it "has suffered little injury beyond the loss of the fleshy envelope. In this and some others I have seen, the three conspicuous salient ridges which traverse the surface longitudinally are shown to be the remains of broad and delicate wings; so delicate that they are generally all torn away with the exception of their thickened bases"†. All this is, I am satisfied, a mistake, which so acute an observer as Professor NEWBERRY would be one of the first to correct, after seeing my sections and specimens. In the valuable memoir published by Dr. HOOKER and Mr. BINNEY the only sections figured are longitudinal ones. The publication in that memoir of figures of transverse sections would probably have prevented the perpetuation of these erroneous notions.

I have not much to add to the descriptions of longitudinal sections of *Trigonocarpon* published by these authors, except on one or two points on which fresh light is now thrown. Dr. HOOKER and his coadjutor have described accurately the general features of these sections, including the two layers constituting the sarcotesta and endotesta. In their figures 7, 8, & 12 they also delineate the organ which apparently represents the lagenostome, but, as might be expected from the date of their publication, without recognizing its significance.

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longitudinal sections as possible out of one of them and of transverse ones out of the other. Out of the first of these, which was less than $\frac{3}{4}$ of an inch in diameter, he obtained for me *nine* beautiful sections, and from the other, less than an inch in length, he obtained no less than eighteen equally fine transverse sections! The more characteristic of each of these two series of sections have been figured in the accompanying Plates.

* Report of the Geological Survey of Ohio, vol. i. Geology and Palæontology, part ii. pl. 42. fig. 1.

† *Loc. cit.* p. 366.

Fig. 95 is the outermost or most tangential of the consecutive series of five vertical sections, selected from the nine of which I have already spoken as being made from one of Mr. NIELD's specimens. There is a small portion of the outermost investment, or sarcotesta, seen at *a*, within which is the denser endotesta at *b*: the minute structure of each of these layers will be referred to presently. The oval outline b' merely represents the complete contour of the inner surface of this endotesta, where it is in contact with the white calcareous spar with which the internal cavities of these seeds are usually filled; *f* is the nucular membrane, which doubtless was originally in immediate contact with the interior of the endotesta, but which has shrunk away from it; whilst *g* is a yet more internal membrane, apparently identical with the perispermic one of the previously described seeds.

Figs. 96 & 97 are two sections made near to the central axis of the seed, so that the long narrow micropyle (*d*) comes into view in both; we also see in each of them the appendage (*c*), on the apical extremity of the nucular membrane (*f*). In each section the inner or perispermic membrane (*g*) has become shrivelled and collapsed. Fig. 98 is a tangential section, which has just avoided passing through the micropyle, and in which the lagenostome (*c*) is still observable; whilst in fig. 99 we return nearly to the conditions seen in fig. 95, but on the opposite side of the seed. The perispermic membrane does not appear in this latter section, as would have been the case with a similar tangential one, made through the right-hand side of either of the figures 97 & 98. Figures 100 to 108 represent an instructive selection of the most characteristic of my eighteen transverse sections, made from one of Mr. BUTTERWORTH's specimens. Fig. 100 has intersected the seed near the base of the micropyle, the section of the open triangular tube of which is seen at *d*. The testa is here very thick in proportion to the diameter of the central canal which it encloses. At each of the angles of the latter the endotesta projects outwards with the sharp angle seen at b' b', the third one having been accidentally broken away. On either side of each of these three projections of the endotesta is another smaller prominent acute angle (b'' b''), whilst midway between each of the three primary angles (b', b') the endotesta exhibits a third rounded prominence (b''', b'''). We thus see that the hard endotesta, deprived of its soft sarcotesta (*a*), had twelve prominent longitudinal ridges, nine of which had sharp keel-like margins, and the remaining three were more rounded and swelling[*]. Though the external outline of the sarcotesta (*a*) is less sharply defined than that of the endotesta, we yet see that to a considerable extent the former follows the latter, the differences seen in the sections being due to shrinkage from some collapse in the soft parenchymatous cells of the outermost layer.

In fig. 101 the section is made a little below the base of the micropyle, and I doubt not that in the recent seed it would have passed through the upper extremity of the nucleus;

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[*] Viewed in the light now thrown upon these structures in *Trigonocarpon*, it seems to me possible that some of Professor NEWBERRY's species of *Rhabdocarpus* (*loc. cit.* pl. 44) may merely be the endotesta of seeds of that genus deprived of their sarcotesta.

hence the triangular aperture (*d*) of the previous figure now exhibits oblate sides. In fig. 102 this enlargement of the nucular cavity becomes yet more conspicuous, and in fig. 103 a further expansion of that cavity has taken place, whilst in its interior we now find the upper part of the nucular membrane *f*. Unfortunately the section has just missed the lagenostome (*c*) of figs. 96, 97, and 98. In fig. 104 the nucular membrane (*f*) is further enlarged, and within it is the perispermic membrane (*g*). Similar arrangements continue to appear in the sections (figs. 105 & 106), save that in them the perispermic membrane (*g*) has collapsed to one side of the nucular membrane (*f*), and the latter is beginning to show a ragged outline on its external surface. In fig. 107 we have passed below the greatest diameter of the seed. The exterior of the nucular membrane is becoming yet more ragged, indicative of its forcible detachment from the inner surface of the endotesta; whilst in fig. 108 we have reached the base of the perispermic membrane (*g*) and approached the chalaza. Parts of two sections successively made yet lower down are represented in figs. 109 and 110, the former in the endotesta and the latter in the sarcotesta. To these further reference will be made.

The letters of reference appended to the various ridges of the endotesta in fig. 100 are similarly attached to the same parts in fig. 103, in which these ridges are very clearly exhibited. Fig. 104 shows with perfect clearness that an inorganic cast of the interior of the cavity of the endotesta made after the nucleus and its two investing membranes had disappeared would exactly correspond with what we see in fig. 94. It is sufficiently easy to discern the real nature of "the three conspicuous salient ridges" spoken of by Professor NEWBERRY. They were portions of the inorganic cast filling the three very narrow slits within the three chief prominent ridges of the endotesta (b', b', b'). The several sections (figs. 101, 104, 105, 107, & 108) show that the three radiating slits in the endotesta are the largest and deepest at the upper parts of the seed, and that they diminish in size as we approach the centre of the seed, below which they grow rapidly less as we approach the chalaza, when they almost disappear—conditions that are identical with what we find in fig. 94. The exterior of the seed exhibits nothing to correspond either with the three thin "broad and delicate wings," or with the smooth convex surface of the "nut" in fig. 94; but the internal cavity of the endotesta displays all these characteristics with the most minute exactness. I think that doubts as to the correctness of my explanation of the common sandstone specimens of *Trigonocarpon olivæforme* and its numerous near congeners can no longer be entertained.

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The minute structures of the various investments of this seed have already been described by Dr. HOOKER and Mr. BINNEY; but it is obvious that they are subject to some variation. Fig. 111 exhibits a portion of a vertical section through the testa, and fig. 112 a similar transverse section through the same tissues; both of them enlarged 62 diameters.

In fig. 111 we see that the sarcotesta (*a*) consists of a parenchyma with very thin cell-walls, and with the cells approximately uniform in size. The endotesta (*b*), on the other hand, consists of cells of various lengths, which are obviously modifications of sclerenchyma. These cells meander throughout the greater part of the thickness of the endotesta in various directions, and the apparent length of the cells, itself a variable quantity, differs according to the direction in which the section has passed through them. But at the innermost portion (fig. 111, *b'*) of this structure all these cells are prosenchymatous, and run parallel with the long axis of the seed. Dr. HOOKER found many of these innermost cells to be barred or spiral ones. Traces of the same structure appear in my sections.

In the transverse section (fig. 112) the cells (*b'*) of the innermost layer of the endotesta are seen to be divided by the section transversely to their longer axes. On the other hand, immediately external to this layer, we find them, as at *b''*, meandering horizontally round the seed. The transverse sections of these latter interlacing cells are also seen in fig. 111, *b''*. The outermost ones in fig. 112 exhibit a strong tendency to be prolonged horizontally and radially as at *b*, where their extremities are in contact with the parenchymatous cells of the sarcotesta (*a*). It is evident that these cells are more elongated in my sections than in those figured by Dr. HOOKER and Mr. BINNEY*.

In most of the sandstone casts like fig. 94, the base of the seed exhibits a small defined ring, occupying the position of the chalaza, in the centre of which is a little point, at which I may presume that a vascular bundle reached the chalaza. My sections, however, exhibit no trace of vascular elements. Fig. 110 represents a transverse section made through the sarcotesta of the specimen figs. 100–108, enlarged 25 diameters. It consists merely of parenchyma with a central aperture, through which, doubtless, the vascular bundle of the chalaza formerly entered the seed. The corresponding part of the endotesta was damaged in making the section of it; hence I cannot identify the upward prolongation of this aperture; but fig. 109 exhibits the aspect of some of the neighbouring sclerenchymatous cells of this region enlarged 108 diameters.

The only region requiring further elucidation is the peculiar structure which appears to represent the lagenostome. In their figures Dr. HOOKER and Mr. BINNEY represent a small thickening, corresponding with *c* in my figures 96, 97, and 98, but no reference is made to this peculiarity in the text of their memoir. Figs. 113, 114, and 115 severally represent these structures, enlarged 15 diameters. In each of these instances the membrane *f* is seen to split into the two layers *f'* & *f''*. In *f'* I can trace no structure; but the inner layer (*f''*) is obviously a thin prosenchymatous tissue, as is so usually the case with these nucular membranes. This peculiar organ so evidently exists in all the specimens of these seeds that its presence cannot be regarded as merely accidental; whilst its position, and the peculiarity of the membrane out of which it is formed, alike indicate its apparent homology with the organ which I have designated the lagenostome. Fig. 113 appears to indicate that the nucleus terminated at the base of this organ, and did not enter into its inner cavity.

I am indebted to JOHN AITKEN, Esq., of Bacup, in Lancashire, for the loan of the

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* *Loc. cit.* plate iv. figs. 13 & 15.

beautiful seed represented in figs. 115 *a* & 115 *b*. It is the only example I have seen from the Oldham nodules resembling M. BRONGNIART's genus *Hexapterospermum*. Specimen fig. 115 *a* represents a side view of the seed of its natural size; and fig. 115 *b* exhibits the same seed enlarged 3 diameters. It differs primarily from *Trigonocarpon* in having six projecting angles instead of three. These ridges, which are very sharply defined, also terminate in a ring of prominent protuberances at the base of the seed, instead of gradually disappearing on approaching the chalaza. LINDLEY and HUTTON described a six-angled seed under the name of *Trigonocarpon Nöggerathii*, which they only distinguish from *T. olivæforme* in being more oblong and in having six angles instead of three. I think there can be no doubt that fig. 115 represents a young form of the same seed. I found in the Museum of the Yorkshire Philosophical Society a sandstone cast of a similar seed from Aldwark, near Rotherham, but it was fully 1½ inch in length. According to M. BRONGNIART's description, the internal structure of his *Hexapterospermum stenopterum* is almost identical with that of our *Trigonocarpon olivæforme*, which plant he separates from the genus *Trigonocarpon* under the name of *Tripterospermum*. This division into three genera of plants which resemble each other so closely, both in their internal organization and in their general external form, is extremely objectionable. The incidental circumstance that one seed dehisced into three segments and another into six, is surely insufficient ground for more than a specific distinction. In like manner he distinguishes his allied genus *Ptychotesta* by a character which my transverse sections unequivocally show to exist equally in *Trigonocarpon olivæforme*. He says "les six ailes qui prolongent les angles de la graine à section hexagonale sont en effet formées, non par une extension du tissu du testa, mais par le testa lui-même replié à l'extérieur. Ces ailes ont ainsi une double paroi identique, pour sa structure et son épaisseur, au testa qui entoure le corps de la graine." Surely this is substantially what we do see in my figure 104 and other sections. All these genera will require to be corrected unless other distinctive characters be found presenting better reasons than we now possess for keeping them separate.

There is great confusion in the nomenclature of these Trigonocarpoid seeds, arising from our inability to determine how far size constitutes a legitimate specific distinction. At Peel Quarry I find considerable variation in the dimensions of what are unmistakable examples of *T. olivæforme*. Excepting in size, I discover no difference between the largest of these specimens and the *T. ovatum* and *T. oblongum* of LINDLEY and HUTTON's 'Fossil Flora.' Professor NEWBERRY evidently recognizes this fact, since the two examples of HILDRETH's *Trigonocarpon triloculare* which he has figured (*loc. cit.* pl. 42. figs. 1–13, 13 *a*) differ in size quite as much as do the *T. olivæforme* and *T. ovatum* of the 'Fossil Flora.' At the same time I cannot see any difference between the latter species and that described by HILDRETH. I have already referred to the similar difference in size between Mr. AITKEN's specimen of *Hexapterospermum* and that in the York Museum. The *T. Dawesii* ('Fossil Flora,' pl. 221), of which I have specimens from Peel Quarry, appears to me to be distinct. In addition to its large size (2¼ inches long) it is *always* a much more oblong seed than is the case with *T. olivæforme*. How far *any* specific distinctions can be ascertained from these *internal* casts of seeds, apart from structure, is extremely doubtful.

The next seeds which I have to describe are those which have long been known by BRONGNIART's name of *Cardiocarpus*. Until recently we were only familiar with the external aspect of various forms and sizes of these seeds, disconnected from their stems. At length the discovery, by Mr. PEACH, of some specimens described and figured by Mr. CARRUTHERS[*] exhibited these seeds in such connexion with the plants known as *Antholithes* as to leave no doubt that the latter are the racemose peduncles, on the secondary peduncles of which some species of these seeds were supported. But it is obvious that *Antholithes* is not the only form assumed by the peduncles of these fruits. Professor NEWBERRY figures one[†] where the seeds are sessile on a long straight peduncle, and Dr. DAWSON figures a small fragment[‡] in which the seeds are similarly arranged; only here each seed is supported upon a short solitary peduncle, devoid of the surrounding appendages characteristic of *Antholithes*.

The genus *Cardiocarpon* was established by BRONGNIART, in his 'Prodrome,' in 1827, for the reception of "Fruits comprimés lenticulaires, cordiformes ou réniformes, terminés par un pointe peu aiguë" (*loc. cit.* p. 87). Oddly enough, his later 'Tableau' contains no notice of it as a distinct genus, but he refers to the seed as probably belonging to the Noeggerathiæ.

All the writers who followed BRONGNIART have spoken of these objects as *fruits*. In his memoir on the Devonian plants just quoted Dr. DAWSON examined these supposed fruits, and "arrived at the conclusion that the *Cardiocarpa* of the type of *C. cornutum* were Gymnospermous *seeds*, having two cotyledons imbedded in an albumen, and covered with a strong membranous or woody tegmen, surrounded by a fleshy outer coat, and that the notch at the apex represents the foramen or micropyle of the ovule" (*loc. cit.* p. 61). In the same memoir (p. 62) Dr. DAWSON further remarks:—"Though I have no doubt that the above is the correct interpretation of *C. cornutum*, I do not regard it as applicable to *all Cardiocarpa*." Mr. CARRUTHERS, in his later memoir, referred to above, speaks of them as fruits invested by a pericarp.

I need scarcely say that this question of seeds *versus* fruits cannot be easily settled, in the case of the fossil examples, so long as Botanists hold such widely divergent views on the same question in relation to the reproductive organs of living Gymnosperms. Whilst EICHLER contends that the seeds of Conifers are really gymnospermous, and STRASBURGER thinks otherwise, it must obviously be difficult to decide the question in the case of fossil forms detached from most of their surrounding organs.

Professor NEWBERRY has figured in his work already quoted a fine series of *Cardiocarpa* from the Carboniferous deposits of Ohio, but none of these display any definite internal structure. M. Brongniart has figured four species from St. Etienne in which the internal organization is preserved. The author says of this genus:—"Sa structure montre une graine orthope, la chalaze correspondant à l'échancrure basilaire, et le micropyle à la pointe opposée; le nucelle a un sommet conique sans apparence de cavité pollinique. Mais ces graines présentent, dans leur testa, deux structures très-différentes, et qui pourraient engager à y distinguer deux genres. Le *C. sclerotesta* offre un testa entièrement dur et nettement limité à l'extérieur. Le *C. drupaceus* présente, au contraire, un tissu très-dense près de sa surface interne, qui passe insensiblement à un tissu à grandes cellules plus transparentes, formant une zone probablement charnue comme celle des graines de Gingko, et présentant même des espaces plus transparents assez régulièrement disposés; correspondant sans doute à des cavités gommeuses aux oléagineuses."

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[*] "Notes on some Fossil Plants," Geol. Mag. vol. ix. no. 2, Feb. 1872.

[†] 'Geol. Survey of Ohio,' pl. xli. fig. 4.

[‡] 'Fossil Plants of the Devonian and Upper-Silurian Formations of Canada,' pl. xix. fig. 227, *a*.

I have found fine examples of this genus in the Oldham nodules, to which Mr. Butterworth has added the fine species to which I have appended his name as a memorial of the services he has rendered to Palæontological Botany, by his diligence in collecting and making valuable sections of the Oldham plants.

Fig. 116 represents a specimen which exhibits much of the external contour of the seed on the side figured, whilst on the opposite surface of the specimen I have obtained the somewhat oblique, longitudinal section represented in fig. 117. Occupying the central part of the former figure, we see a triangular nucleus with rounded angles. A prominent, obtuse, longitudinal ridge separates most of the surface of this nucleus into two triangular areas, which slope away from the ridge in each direction towards a thick, rounded, and very prominent margin, *n'*, *n'*, which surrounds the two sides and the apex of this nucleus. At the chalazal extremity the central longitudinal ridge dichotomizes, and is prolonged in each direction to the two obtuse latero-posterior angles, *n''*, *n''*, of the nucleus, thus defining a third or posterior area, *i*, which, though triangular, like the two lateral ones, is so much shorter as almost to constitute a truncate chalazal base of the entire nucleus. This nucleus is the thickest at the prominent point where the longitudinal and transverse ridges meet. Its superficial surface is thus seen to constitute a depressed cone, of which the apex is excentric. The maximum length of the nucleus, so far as it is visible in this specimen, is about ·11, and its diameter across the obtuse apex of its prominent conical surface is about the same. External to the nucleus we can readily discern, on each side of the seed, two investing membranes, an outer (*a*) and an inner one (*b*), which taper away together, forming a long micropylar prolongation, *c*.

Unable to obtain a transparent section, I ground away the opposite side of this fine seed, and obtained the result seen in fig. 117, which is again enlarged 15 diameters. Either the section is slightly oblique or, as other specimens indicate, the seed has been liable to become somewhat twisted instead of being always symmetrical. It now shows very clearly the distinction of its various parts. At *n'* we have a central nucleus, of which some of the parenchymatous cells are preserved. These cells, as in all the species of *Cardiocarpon* in which I have found this perispermic tissue, are of large size, some of them having a mean diameter of ·004; a cluster of them, enlarged 50 diameters, is represented in fig. 120.

This perisperm is invested by a very thin membrane, *g*, in which I can find no structure, and which, I conclude, is identical with the perispermic membrane of the previously described seeds. It is rounded at its apex, not being prolonged into the micropyle, resembling in this respect the Cardiocarpons figured by M. Brongniart*. External to this perispermic membrane we have two layers, which, though they accompany each other in their course, differ in their structure. The inner one (*b*) is very thin and delicate, consisting of a single layer of thin-walled prosenchymatous cells. Superiorly it is prolonged through the micropyle, and, in this instance, extends beyond the outer layer at *b'*; but this is doubtless an accidental result. At its base (*b''*) it has evidently been closely united with the chalazal tissues. The outermost membrane, or exotesta (*a*), is dense and well defined; though in this instance its parenchymatous structure is not very clearly shown. Inferiorly it is prolonged into a funiculus or peduncle (*k*), whilst at its apical extremity it projects in a long tubular micropyle.

I cannot determine with certainty the relation which the inner membrane (*b*) in this species bears to those seen in the other seeds which I have described. In its structure it corresponds closely with what, following M. Brongniart, I have designated the nucular membrane. But in its prolongation to the extreme tip of the micropyle it differs completely from the nucular membrane of the other seeds. Hence I am inclined to regard it as a thin and delicate endotesta. The other specimens to be described confirm me in this conclusion, because, though the perispermic structures have entirely disappeared from them, this membrane remains in perfect integrity. Thus fig. 118 represents a transverse section of a specimen enlarged 22½ diameters, its mean size being about ·1. The exotesta (*a*) is very sharply defined both externally and internally, and has a mean thickness of about ·007. In the body of the seed, where it has invested the nucleus, it expands broadly into two rounded wings; but at the apex of the nucleus it has rapidly contracted into the tubular prolongation *a'*, forming the micropyle, and which has been curved laterally near its exostomal extremity, *d*. The inner membrane or endotesta (*b*) exhibits the same delicate prosenchymatous structure as is seen in the corresponding membrane in fig. 117. In like manner it is firmly united with the exotesta at the chalaza (*i*), and is prolonged as an inner layer of the micropyle to the point *d*. The nucleus and nucular membrane are wanting in this example.

Fig. 119 represents one of the finest specimens I have met with, so far as the preservation of the tissues of the testa are concerned. It is enlarged 22 diameters. Whether it is a distinct species from figs. 116 & 118, or whether it is merely a large example of the same, I am unable to say; but it is more than twice the diameter of fig. 116, and more than three times that of fig. 118. In every other respect it corresponds closely with those figures. We now see that the exotesta (*a*) consists of innumerable very minute cells with dense cell-walls, and which in some parts, as at *a'*, assume a prosenchymatous form. Thick at the base of the seed, it becomes thinner as it rapidly contracts in size to form the prolonged micropyle, *d*, which is obviously curtailed of much of its normal length. At the chalaza (*i*) the cells of the membrane curve away on either hand, in lines which bend, first, downwards, then upwards and outwards from its junction with the endotesta, *b*. This latter membrane is again seen to be thin, and composed of a single layer of delicate prosenchymatous cells, excepting at the chalaza (*i*), where they appear to be parenchymatous; but it is possible that, at this point, the section may not be exactly central, and that, consequently, the cells may be intersected across their narrow diameters. At the opposite extremity the endotesta is prolonged (*b'*) through and beyond the micropylar extension of the exotesta; but the tissues of this region have obviously been injured. The nucleus and perispermic membrane are again wanting in this example. All the three specimens here described are from the Oldham nodules.

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* 'Études sur les graines fossiles,' pl. xxi. fig. 1.

The application of correct specific names to these Cardiocarpons is not easy. Mr. CARRUTHERS, to whom I showed the original of fig. 116, considers it to be identical with his *C. anomalum*[*]. Though far from certain, not having seen Mr. CARRUTHER'S specimen, the seed which I have described may remain under that name, upon the authority of its author.

I have found in the Oldham nodules several specimens of a small compressed *Cardiocarpon*, in addition to a portion of one obtained by Mr. BUTTERWORTH. Their outlines were not very clearly defined, especially at the micropylar extremity; but fig. 121 represents the general aspect of most of the specimens, allowance being made for some uncertainty respecting the external contour of the apex. There is in all a smooth, flattened nucleus, with an investing layer, *a*, which extends considerably beyond the nucleus nearly all round the seed, but which is especially somewhat prolonged at the apex. In Mr. BUTTERWORTH'S fragment there is a slight longitudinal groove running down the centre of the surface of the nucleus, as represented in fig. 121.

Fig. 122 represents one of a number of seeds on a piece of shale from the Newcastle Coal-field, and which I presume is the *Cardiocarpum acutum* of LINDLEY and HUTTON. It is possible that this may be the same species as fig. 121, in which case both may probably be identical with the *C. Lindleyi* of CARRUTHERS[†]. But if so, the bifid extremity of the fruit in Mr. CARRUTHERS'S plant, and in my fig. 122, has probably been the result of pressure, which has split open the patulous extremity of the micropyle.

Fig. 123 represents a central vertical section of the upper half of a seed made in the plane of fig. 121. We here only see an exotesta, *a*, enclosing a large internal cavity; its upper extremity contracts gradually into a short and narrow micropyle, *d*, but which terminates in a trumpet-shaped mouth or exostoma at *d'*. We have shrivelled portions of an internal membrane at *g*, but I cannot ascertain what it is. Fig. 124 represents a transverse section of the same seed made a little below the base-line of fig. 123. We here see the lenticular form of the cavity enclosed by the testa, *a*; and though the latter is imperfect, enough remains to show that it, too, had a lenticular outline. Within this testa we now find two membranes, of which the inner (*g*) probably represents the perispermic membrane. Fig. 125 is one surface of another transverse section, made parallel to the last, but near the base of the seed. Being the opposite surface of the thick section, fig. 124, its position is reversed—the thick layer of the exotesta, *a'*, being now on the upper instead of the lower side of the figure. The parenchyma of the chalaza is seen at *i*, on each side of which are two basal prolongations of the cavity enclosed within the testa, and which project below the chalaza, indicating the cordate shape of the endotestal cavity at the chalazal extremity of the seed.

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[*] 'Notes on some Fossil Plants' &c., fig. 3.

[†] *Loc. cit.* fig. 182.

Figs. 126, 127, & 128 are three serial longitudinal sections of another specimen made vertically to figs. 121 & 123. The first of these is a central one. Unfortunately the base of the specimen was lost; but we now find that the testa is divisible into two layers. The outer one (*a*) is more or less disorganized, but various indications show that it consisted of a parenchymatous tissue. Thus at *a'* we have a line of cells which might easily be mistaken for an epidermal layer; but we shall find similar cells in fig. 127 at *a'*, and still more at *a''*, occupying the innermost portions of the same tissue. Combining these sections we appear to have evidence that the exotesta consisted of a coarse parenchyma, many of the cells of which have a diameter of ·003. Within this is the more dense endotesta, fig. 126 *b*, which is here about ·01 in thickness, and consists of parenchyma, the cells of which are of much smaller size than those of the exotesta. At the apex of the nucleus these united portions of the testa suddenly contracted to form the micropyle, *d*, which was the narrowest at its nucular extremity, and gradually expanded into a trumpet-shaped aperture at its open mouth, corresponding in this respect with fig. 123; but it must be remembered that the latter section is made along a line between the points *y*, *y* of fig. 124, whilst fig. 126 is made across the shorter diameter of the seed from *x* to *x*. At fig. 126, *b'*, *b'*, we see a tendency to the separation of a thin membrane from the interior of the endotesta, which may be the same as fig. 124, *b*. The tissues enclosed within the testa have become much contracted and shrivelled, especially towards the apical half of the seed—a condition which was equally seen in fig. 123. In fig. 126, *f*, we have an outer membrane, and at *g* there appears to be an inner one. The former is shrivelled up, as in fig. 123, to little more than a mere line at the upper half of the nucular cavity, whilst inferiorly the two membranes *f* & *g* unite to invest the perisperm *n*. Thus in both the specimens examined all traces of the nucleus (*n*) seem to have wholly disappeared from the upper half of the interior of the seed, whilst more of it remained at the lower half, where, both in figs. 124–126 & 127, we see clear indications of what may be regarded as a double perispermic membrane, *f*–*g*. Unfortunately none of my sections show whether or not the nucleus, with its investing membrane, originally occupied the interior of the upper part of the membrane *f'*, or whether the latter was largely detached from the perispermic membrane to form a lagenostomal cavity. This point requires further investigation when favourable specimens can be obtained.

Fig. 127, which is a section made along the line fig. 121 *x–x*, shows the endotesta, *b*, extending upwards into the thin lenticular expansion of the testa which borders each side of the micropyle; and the same condition is seen in fig. 128, which is a similar section made through the testa bordering the nucular cavity, *i.e.* along the line *y* in fig. 121. Fig. 128 is enlarged 50 diameters, to show the cells of the endotesta (*b*) arranged in long vertical lines, whilst the exotesta (*a*) exhibits its usual disorganized condition. The line *a'* indicates the former boundary of the latter tissue, much of which has disappeared. The length of the specimen, fig. 126, has been about ·21, and the diameter of fig. 124 about ·25. All the specimens agree in giving to this species much of the general dimensions and contour of the *Cardiocarpum acutum* of LINDLEY and HUTTON, and of the *C. Lindleyi* of Mr. CARRUTHERS, excepting that the latter observer describes his seed as having a central longitudinal ridge, which my specimens certainly have not. Since these differences exist, it may be well to distinguish my type under the name of *Cardiocarpon compressum.*

Since the above descriptions were written, Mr. A. BUTTERWORTH has found, near Oldham, the specimen from which he has prepared the section represented in fig. 128*. It is a transverse one, made immediately above the cordate base of the lenticular seed. In it we have a thick testa, *a*, within which we find the very distinct membrane *f*, which, I presume, is identical with *f* in fig. 124. This, in turn, encloses the shrivelled remains (*g*) of what, I presume, has been a perispermic membrane identical with fig. 124, *g*; but between the two membranes *f* and *g* we have a small number of scattered, free cells, *l*, *l*, which closely resemble the supposed pollen-grains seen in other seeds. Unfortunately the micropylar extremity of Mr. BUTTERWORTH's specimen was lost; hence I cannot determine whether it represents a distinct species or is identical with the *C. compressum*, but if distinct the two types are very closely allied. Supposing the objects *l*, *l* to be pollen-grains, we obtain some light respecting the cavity into which these grains are received in *Cardiocarpon*, and which plays the part of the lagenostome seen in the other seeds. *A priori*, the organization of figs. 116, 117, 118, 119, & 126 would suggest the inference that the pollen-grains would pass down the micropyle through the tubular part of the membrane (*b*) of the first four figures, and into the cavity bounded by the membrane (*b'*) in fig. 126, in each of which cases the grains would be brought into immediate contact with the nucular membrane. If this is a correct explanation, these seeds of *Cardiocarpon* differ little from the simpler types of Coniferous ovules seen in *Juniperus* and *Callitris*. Thus, in each of the figs. 117 & 126, the pollen would reach the triangular cavity (*x*) without the intervention of the more complex arrangements of the lagenostome seen in many of the other seeds which I have described. In their internal organization, and especially in the great length of the micropyle, some of these seeds bear a strong resemblance to the ovules of *Welwitschia*†.

Mr. BUTTERWORTH was also fortunate enough to find in one of the Oldham nodules the elegant *Cardiocarpon* represented in figs. 129, 130, & 131. It consists of two parts, the actual seed and a slender peduncle. The former is about ·4 in length and the latter ·25, whilst its diameter at its widest point, *i. e.* near its base, is nearly ·2. Fig. 129 represents the natural size of the specimen; in fig. 130 it is enlarged six diameters. It is lanceolate in form; a prominent ridge which runs up its centre is thick at the base, and almost reduced to a line at the apex. On each side of this ridge is a very regular, parallel groove, external to which again are two broad elevated surfaces, rounded and prominent at the base, but with somewhat flattened surfaces, and which extend to the lateral margins of the seed as well as to its apex. The narrow peduncle is nearly as long as the seed.

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† "On *Welwitschia*, a new Genus of Gnetaceæ," by JOSEPH DALTON HOOKER, F.R.S., Transactions of the Linnean Society, vol. xxiv. pl. ix. figs. 11 & 12.

Owing to the opposite side of this specimen being crushed, I could not obtain a good section revealing its internal structure; but it clearly shows a double membrane, the outer one of which, I presume, will correspond with the membrane *b* in the sections of the other *Cardiocarpons*, and the inner with the perispermic one. Both these membranes appear to have extended to the apex of the seed. The true testa seems to have been a very thin one.

A second fragment of a slightly larger specimen of the same species, also found by Mr. BUTTERWORTH, is valuable because it exhibits much of the cellular parenchyma of the perisperm. Some of these cells from near the perispermic membrane are represented in fig. 131. In this figure, some of the cells nearest the perispermic membrane, *g*, appear loose and detached; but in a second section which I have obtained they are all compactly united as in ordinary parenchyma, and as seen in the lower part of fig. 131. Many of these cells have a mean diameter of ·006, which is half as large again as those of *C. anomalum*, represented in fig. 120. This *Cardiocarpon* differs from all those of which I can discover any published account. I have therefore given to it the name of *C. Butterworthii*, as a memorial of the valuable aid which its discoverer has rendered me, by supplying me with specimens of the Oldham fossils, as well as by his quickness in detecting new forms and his skill in preparing sections of them.

Figs. 132, 133, & 134 represent three seeds from a slab of shale from Swinehill Colliery, near Stonehouse, Lanarkshire, for which I am indebted to JOHN YOUNG, Esq., of the Glasgow Hunterian Museum*. The seeds are scattered over several large slabs of shale in great numbers, no two of them being exactly alike, save in the fact that they are all compressed, as well as more or less fluted longitudinally. They also exhibit the appearance of a surrounding margin; but I suspect that this may merely have been a result of pressure acting upon a thick testa, which would resist that pressure where it was folded back upon itself. These seeds appear to me to agree with the figure and description given by Dr. DAWSON of his *Cardiocarpum tenellum*†. I scarcely think that they belong to the genus *Cardiocarpon*; but this cannot be determined at present; hence I leave them provisionally with the name which Dr. DAWSON has assigned to them.

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* Mr. YOUNG informs me that these specimens were discovered by Mr. JOHN SMITH, of Stables, Kilwinning, in Ayrshire. The coal from the roof of which they were obtained is the Virtue-Well coal, one of the seams of the upper series of Coal-measures.

† 'Report on the Fossil Plants of the Lower Carboniferous and Millstone-grit Formations of Canada,' pl. vi. fig. 50, p. 28.

The only seeds which remain to be noticed are the two represented by figs. 135, 136, & 137. These are from a bed of fossiliferous red shale which I discovered in 1837* near the top of the Ardwick beds at Manchester. These deposits correspond to the Limestones of Lebotwood, in Shropshire, and constitute the uppermost of the Carboniferous beds occurring in this country. The shale abounds in plants (including *Neuropteris cordata*, the fern so characteristic of the uppermost of the Carboniferous strata), in *Lepidodendra*, and in *Asterophyllites*.

Fig. 135 represents one of these seeds of its natural size.

Fig. 136 is the same specimen, enlarged four diameters; and fig. 137 is another seed, from the same slab of shale, also enlarged four diameters. Both these specimens are imperfect; but the seed has obviously been a somewhat winged one, thin, compressed, and leaf-like in structure. The rounded leaf-like apex is best seen in fig. 137, whilst the thin membranous base is better seen in fig. 136. The median surface of each seed is characterized by a series of elevated irregularly branched ridges. According to M. GRAND'EURY, these seeds belong to the genus *Polypterocarpus*, and closely resemble the *P. caudatus* found in the French Coal-measures. This latter is a double seed, resembling in its form the samara of an ash. My two portions being found very near to each other upon the same slab of shale, it is quite possible that the base (*i*) of fig. 137 may have been broken off from the part *i* in fig. 136, the two being portions of one seed.

The descriptions and figures which I have here given of such seeds as I have been able to obtain from our British Carboniferous rocks will suffice to show how close is the resemblance of the types which prevail here to those described by M. BRONGNIART from the French rocks of the same formation; but there is one remarkable difference. With us the seeds are all of small size, excepting in the case of *Trigonocarpon* and *Hexapterospermum*, whereas the St.-Étienne specimens are mostly of large dimensions. I am indebted to my friend M. GRAND'EURY for a valuable series of these latter specimens, and am struck with the contrast, in this respect, between his examples and mine.

I will not venture at present upon any speculations respecting the affinities of these seeds, further than to say that they must have belonged to Phanerogamic plants, and that the more they are studied, the more nearly they seem to approach to those of the gymnospermous type of organization; at the same time they show but few exact affinities with any known seeds. How far they may ultimately be found to resemble the fertilized ovules of the Cycads, as M. BRONGNIART's last communication made to the Academy of Sciences† seems to suggest, time and further study alone can show. Still less can we at present associate them with any of the stems and leaves with which students of Carboniferous vegetation are so familiar. I cannot, for instance, follow Dr. DAWSON and Prof. NEWBERRY in identifying *Trigonocarpon* with *Sigillaria*, merely because the former seeds have sometimes been found accumulated near the bases of the erect stems of the latter. We have numbers of magnificent stems of *Sigillariæ* in Britain, and yet I find no one instance recorded of the two objects being so associated in our British deposits. Evidence of this kind requires to be received with the utmost caution. In time, I doubt not, some of these seeds will be found attached to peduncles from the structure of which much will be learnt; until then we must be content to wait. One lesson we may certainly learn from the numerous species of these seeds with which we are already familiar, viz. that there were, in the Carboniferous forests, many Gymnospermous stems clothed with foliage, of which we have not yet discovered any traces, probably because, as has more than once been suggested, these Gymnosperms did not flourish upon the low swampy grounds which were the homes of the great mass of the coal-producing plants.

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* "On the Limestones found in the vicinity of Manchester," Phil. Mag. ser. 3, xi. When this shale was discovered I was under the guidance of Mr. MELLOR, the intelligent superintendent of the "Ardwick Limestone" pits, in the watercourse of one of which the shales were met with.

† Comptes Rendus, tome lxxxi. no. 7.

M. BRONGNIART'S observations on the fertilized ovules of *Ceratozamia Mexicana* and *C. Ghiesbrechtii*, just referred to (*loc. cit.* p. 305), show that, in them, the apical extremity of the unfertilized nucule projected as a mamilliform prolongation which occupied and filled much of the micropyle. My *Lagenostoma physoides* (figs. 77 & 78) exhibits a similar termination to the nucleus. M. BRONGNIART concluded that the mamilliform projection of the nucleus became absorbed, forming, by the disintegration of its cells, the "cavité pollinique" of his memoir, and which I have termed the lagenostome. If this is the case, the similar mamilla in figs. 77 & 78 cannot be the same structure as is seen in the Cycadean ovules, because the narrow prolongation of its upper part is here retained in its integrity, whilst the lagenostome (figs. 77, *c*, & 78, *c*) is superadded to and not formed out of that structure*. M. BRONGNIART'S idea that all these seeds are Cycadean rather than Coniferous must also await further inquiry before we can either accept or reject it. If the grains discovered in several of the lagenostomes are truly pollen-grains, their presence indicates that the seeds containing them were young ones. On the other hand, the structure of their testæ shows them to be true seeds and not ovules. Unfortunately the nucleus has almost always disappeared; and where traces do remain we have as yet seen nothing beyond a cellular perisperm. Further research may, however, reveal other examples in which traces of the peculiar and characteristic endosperms of the Conifers and Cycads may be detected. It is very desirable that all who are in a position favourable to the prosecution of such researches should keep this important possibility steadily in view.

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* My latest studies seem to favour M. BRONGNIART'S conclusions thus far, viz. that the central organ of each of these seeds which, in the course of the memoir, has been termed "the nucleus," may really be the endosperm found in the interior of the original nucleus, the tissues of the latter structure having been entirely absorbed. In this case the only traces of the original contour of that nucleus are now found in what I have termed "the nucular membrane" and its lagenostomal prolongations. If this be correct, it then becomes probable that the remarkable cluster of parenchymatous cells seen in the interior of the "lagenostome" or "cavité pollinique" of so many of my specimens, and the origin of which is so difficult to explain, is neither more nor less than the remains of the cellular tissue of the uppermost extremity of the original nucleus, which primarily formed its narrowed apical extension occupying the micropyle, and which was not absorbed when the rest of the nucleus disappeared. The acceptance of these conclusions would bring my observations upon the Carboniferous forms into close harmony alike with those of M. MIQUEL ('Archives Néerlandaises,' t. iii., 1868) and of M. BRONGNIART upon the fertilized and unfertilized ovules of living Cycads. Of course the conclusion would follow that the affinities of these fossil forms is Cycadean rather than Coniferous.

Whilst thus avoiding all attempts definitely to associate these seeds with individual plants, we must not overlook the fact that most of them have been obtained either from the thin upper foot coal-seam of the Ganister beds, from which so many of the finest of our Oldham plants have been derived, and that, consequently, we naturally look to those plants to furnish the parent stems on which these seeds grew. But they fail to show any structures with which the seeds can reasonably be associated. The only stems which we yet possess that can with any degree of probability be identified with the seeds are the *Dadoxylons*. BRONGNIART thinks that the *Nœggerrathiæ* may have been the parents of some of them, especially of *Cardiocarpon*; but unless the *Cordaites* were *Nœggerrathiæ* we find no trace of that group in the Oldham nodules; and, as already remarked, our chief specimens of *Dadoxylon* have been derived from the marine beds of the Ganister series and of Coalbrookdale, into which they had obviously been drifted, and in which the seeds are extremely rare.

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Rachiopteris corrugata.

Fig. 1. Small portion of the external surface of the cortex, with its transverse ridges and furrows. Enlarged 7½ diameters.

Fig. 2. Transverse section of the lower extremity of the fragment of a stem. Enlarged 1½ diameter.

Figs. 3 to 12. Series of similar sections to fig. 2, arranged numerically in their ascending order. Fig. 4 is enlarged 3 diameters, the remainder 1½ diameter.

The letters of reference throughout this series indicate the following structures:—

a. Medulla. *b.* Axial vascular cylinder. *c.* Primary vascular bundles given off from the axial cylinder, probably to petioles. *d.* Secondary vascular bundles given off either from the primary bundles (*c*) or from the axial cylinder. *e.* Inner cortical tissue. *f.* Outer cortical tissue. *g.* A branch or petiole separating from the main stem and enclosing a primary vascular bundle. *h*. Smaller divergent petiole, or root, containing one of the secondary vascular bundles.

Fig. 13. Central axial vascular cylinder. Enlarged 18½ diameters.

a. Medulla composed of cells intermingled with small vessels. *b*. Seven clusters of vessels blending peripherally into an almost continuous vascular zone. *g*. Bundle-sheath composed of small vertically elongated cells.

Fig. 14. Longitudinal section of the axial vascular cylinder.

a. Medulla. *b*, *b*. Vascular cylinder. *e*. Cells of the cortical parenchyma. *g*. Bundle-sheath. Enlarged 18½ diameters.

Fig. 15. Transverse section of vessels (*b*) of the axial cylinder exhibiting the tylose cells (*i*) with which each vessel is filled. Enlarged 100 diameters.

Fig. 16. Longitudinal section of a similar vessel.

i. Tylose cells. Enlarged 100 diameters.

Fig. 17. The primary vascular bundle, fig. 4, *c*.

g. The bundle. *c*. Inner bark. *g″*. The bundle-sheath.

Fig. 18. Transverse section of a detached petiole.

c. Central vascular bundle. *e*. Inner cortex. *f*. Outer cortex. Enlarged 7½ diameters.

Fig. 19. The portion (*b′*, *b′*) of the vascular cylinder of fig. 6.

b′, *b′*. A cluster of vessels preparing to separate themselves as a primary vascular bundle. Enlarged 18½ diameters.

Fig. 20. Similarly enlarged representation of the projection *b′* from the central cylinder of fig. 7. Enlarged 18½ diameters.

a. Medullary cells. *b*. Cluster of axial vessels. *b′*, *b″*. Vessels separating from the axial cylinder *b*. *d*. Secondary bundle. *e*. Cells of inner cortex. Enlarged 18½ diameters.

Fig. 21. Oblique longitudinal section of part of a central vascular axis.

b. Vessels of the axial cylinder. *c*. A primary bundle departing from the axial cylinder. *d*. A secondary bundle. *e*. Parenchyma of the inner bark. *f*. Outer bark. *g*. Bundle-sheath. Enlarged 6 diameters.

Fig. 22. Cells and vessels from fig. 19, *b′*. Enlarged 62 diameters.

Fig. 23. Transverse section of the primary petiolar bundle, fig. 5, *c*. Enlarged 18½ diameters.

d. A secondary bundle preparing to branch off from the primary one.

Fig. 24. Transverse section of the secondary bundle, fig. 6, *d′*, now becoming detached as a small petiole or root and invested by its cortical tissues. Enlarged 18½ diameters.

Fig. 25. Section of a sporangium of a Fern. Enlarged 75 diameters.

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Fig. 26. Single spore of fig. 25, enlarged 320 diameters.

Fig. 27. Lateral view of the base of a sporangium of a Fern allied to the Gleicheneaceæ or Schizeaceæ. Enlarged 75 diameters.

a. Base of attachment.

Fig. 28. Inferior surface of a similar sporangium to fig. 27. Enlarged 75 diameters.

a. Base of attachment.

Fig. 29. Transverse section of a similar sporangium, intersecting the annulus and filled with sporangia.

Fig. 30. Transverse section of a similar sporangium made near its apex at the upper margin of the annulus. Enlarged 75 diameters.

Figs. 31 & 32. Sections of two of the spores of fig. 29. Enlarged 320 diameters.

a. Spore-wall. *b.* Primordial utricle.

Gymnospermæ.

Fig. 33. Transverse section of the ligneous zone (*b*) and the outermost part of the medulla (*a*) of a young twig of a *Dadoxylon* from Coalbrookdale. Enlarged 50 diameters.

Fig. 34. Transverse section of the bark of the same specimen.

b. Outermost fibres of ligneous zone. *d.* Inner bark. *e.* Outer bark.

Fig. 35. Vertical section of the structures seen in fig. 33. Enlarged 50 diameters.

a. Medulla. *b.* Ligneous zone. *b'.* Medullary sheath.

Fig. 36. Vertical section of fig. 34. Enlarged 50 diameters.

b. Outermost fibres of ligneous zone. *d.* Inner bark. *e.* Outer bark.

Fig. 37. Barred vessels of the medullary sheath of fig. 35. Enlarged 270 diameters.

Fig. 38. Radial section of portions of two fibres of the ligneous zone of fig. 35.

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Fig. 39. Similar representation to fig. 38, but without any medullary ray.

Fig. 40. Tangential section of parts of four fibres with four medullary rays (*f*) and interfibrous beaded lines (*g*), which are sections of the margined disks seen on the surfaces of figs. 38 & 39.

Fig. 41. Vertical section of the Sternbergian pith of a *Dadoxylon* from near Oldham. Natural size.

Fig. 42. Transverse section of the pith and ligneous zone of a large branch of a *Dadoxylon* from South Owram, near Halifax. Natural size.

Fig. 43. Vertical section of the stem fig. 42. *a.* Medulla. *b.* Ligneous zone.

Fig. 44. Transverse section of a small branch of a *Dadoxylon* from near Oldham. Enlarged 6 diameters.

a. Medulla. *b.* Woody cylinder. *d.* Inner bark. *g*, *g*. Pair of bundles going off to leaves?

Fig. 45. Vertical section of fig. 44. Enlarged 6 diameters.

a. Sternbergian pith. *b.* Ligneous cylinder. *d.* Inner bark disorganized.

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Fig. 46. Transverse section of a small branch of a *Dadoxylon*, exhibiting concentric lines of growth. Enlarged 4½ diameters.

g, *g*. Pairs of leaf-bundles.

Fig. 47. Portion of a radial section of the specimen fig. 44, exhibiting a foliar bundle (b') departing from the innermost layer of the woody zone. Enlarged 15 diameters.

a, *a*. Thickened outermost margins of the Sternbergian medullary disks.

Fig. 48. Tangential section of a portion of the ligneous zone of fig. 44, exhibiting two of the foliar bundles (*g*, *g*) intersected transversely. *b*. Vessels of the ligneous zone. Enlarged 15 diameters.

Fig. 49. Tangential section of a portion of the ligneous zone of the Coalbrookdale twig (figs. 33–40), exhibiting some of the fibres undergoing deflection to form a foliar (?) bundle. Enlarged 62½ diameters.

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Fig. 51. Part of a wood-fibre of fig. 50, showing the surface parallel to the medullary rays. Enlarged 270 diameters.

Fig. 52. Part of a fibre, showing the surface parallel to the medullary rays, from Oldham. Enlarged 270 diameters.

Seeds of Gymnospermæ.

Fig. 53. *Lagenostoma ovoides*. Obliquely transverse section through the upper extremity of the micropyle of Mr. BUTTERWORTH's specimen.

a. Exotesta. *d*. Orifice of lagenostome. *e*. Part of the "canopy" of the lagenostome.

Fig. 54. Second section of the same specimen made a little below the apex of the seed.

a. Exotesta. *c*. Lagenostome. *e*. Folds of canopy.

Figs. 55 & 56. Third and fourth sections of the same, made yet lower down.

Fig. 57. Portion of the testa (*a*) and canopy (*e*), from a section intermediate between figs. 55 & 56.

Fig. 58. Tangential section of a second seed of *Lagenostoma ovoides*, made midway between its centre and its lateral surface.

a. Testa. *f*. Nucular membrane. *g*. Perispermic membrane. Enlarged 15 diameters.

Fig. 59. Second tangential section of fig. 58, made nearer to its centre.

a. Testa. *c*. Lagenostome. *e*, *e*. Canopy. *f*. Nucular membrane. *g*. Perispermic membrane. *h*. Parenchyma of the interior of the lagenostome. Enlarged 15 diameters.

Fig. 60. Vertical section made through the centre of fig. 58.

a. Testa. *c*. Lagenostome. *d*. Micropyle. *e*, *e*. Canopy. *f*. Nucular membrane. *g*. Perispermic membrane. *h*. Parenchyma of the lagenostome. Enlarged 15 diameters.

Fig. 61. Tangential section of the same made nearly in the plane of fig. 59, but on the opposite side of the seed.

a. Testa. *c*. Lagenostome. *e*, *e*. Canopy. *f*. Nucular membrane. *g*. Perispermic membrane. *h*. Parenchyma of lagenostome. Enlarged 15 diameters.

Fig. 62. Upper part of the section, fig. 61. Enlarged 80 diameters.

Figs. 63 to 68. Series of longitudinal sections of a third specimen of *Lagenostoma ovoides*, of which fig. 67 is the central one, whilst fig. 68 exhibits the chalaza (*i*) and what appears to have been a funiculus, *k*. Enlarged 15 diameters.

Fig. 69. Upper part of fig. 67, enlarged 80 diameters.

a. Exotesta. *b*. Three folds of the canopy. *c*. Lagenostome. *f*, *f*. Nucular membrane probably united to the perispermic one.

Fig. 70. Imperfect upper or apical portion of a fourth seed of *Lagenostoma ovoides*, with pollen-grains (*l*) lodged in the interior of the lagenostome, between its outer wall and the lagenostomal parenchyma.

Fig. 71. Transverse section near the apex of Mr. NIELD'S specimen of *Lagenostoma ovoides*. Enlarged 15 diameters.

a. Exotesta. *d*. Lagenostome. *e*. Canopy.

Fig. 72. Transverse section of the seed, fig. 71, made across the centre of the lagenostome.

a. Testa. *c*. Membranous wall of the lagenostome. *e*. Canopy. *h*. Parenchyma of lagenostome. *l*. Pollen-grains. *n*. Vertical columns of a peculiar cellular tissue. Enlarged 30 diameters.

Fig. 73. Parallel section to fig. 72, made inferiorly to that figure. Enlarged 30 diameters.

g. Apex of the perispermic membrane.

Fig. 74. Prosenchymatous cells from the membranous wall of the lagenostome in fig. 70. Enlarged 175 diameters.

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Fig. 76. Vertical section through the dried endotesta and nucleus of the seed of a *Salisburia Adiantifolia*.

a. Endotesta. *f*. Nucular membrane. *c*. Cavity between the divided layers of the nucular membrane.

Fig. 77. Vertical section through the centre of a seed of *Lagenostoma physoides*.

b. Prosenchymatous endotesta (?). *c*. Lagenostome. *d*. Micropyle. *e*. Folds of canopy. *n*. Perisperm. Enlarged 15 diameters.

Fig. 78. Tangential longitudinal section of fig. 77.

Fig. 79. Enlarged representation of part of a section made near to fig. 77.

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Fig. 80. *Conostoma oblonga*. Vertical section.

a. Testa. *c*. Cavity, probably occupied by a prolongation of the nucleus. *f*. Nucular membrane. *g*. Perispermic membrane. *h*. Parenchyma of the lagenostome? Enlarged 15 diameters.

Fig. 80*a*. Vertical section of the detached apex of the testa of fig. 80. Enlarged 35 diameters.

Fig 80*. Tangential section of fig. 79 passing outside the parenchyma of the lagenostome.

Fig. 81. Tangential section of fig. 80 made yet more peripherally than fig. 80*.

Fig. 82. *Conostoma ovalis*. Longitudinal section.

a. Testa. *b*. Endotesta. *f*. Nucular membrane. *g*. Perispermic membrane. *h*. Parenchyma of the lagenostome? *n*. Perisperm. Enlarged 22 diameters.

Fig. 83. Upper part of fig. 82.

a. Testa. *b*. Endotesta. *c*. Lagenostome? *c'*. Prosenchymatous wall of the lagenostome, with its inverted funnel-shaped entrance. *f*. Nucular membrane. *f*, *g*. Nucular and perispermic membranes united. *h*. Parenchyma of the lagenostome? *n*. Nucleus. Enlarged 60 diameters.

Fig. 84. Oblique section of a second specimen made in the direction of the line, fig. 82, *xx*.

a. Testa. *c*. Lagenostome? *c'*. Walls of the funnel-shaped lagenostome. *f*. Divisions of the nucular membrane. *h*. Parenchyma of lagenostome. Enlarged 22 diameters.

Fig. 85. Small seed allied to *Conostoma*. From Mr. AITKEN'S Cabinet. Nat. size.

Fig. 86. Vertical section through the centre of Mr. BUTTERWORTH'S specimen of *Conostoma oblonga*.

a. Testa. *c*. Cavity, corresponding to *c* in fig. 79. *d*. Micropyle. *f*. Subdivisions of the nucular membrane enclosing the lagenostomal cavity with its parenchyma, *p*. *f*, *g*. Nucular and perispermic membranes united?

Fig. 87. *Conostoma intermedia*. Longitudinal section, probably not quite central. Enlarged 15 diameters.

a. Testa. *b*. Endotesta? *f*. Nucular membrane. *h*. Parenchyma; probably a remnant of the perisperm.

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a. Sarcotesta. *b*. Hard endotesta. *c*. Lagenostome. *d*. Micropyle. *f*. Nucular membrane. *g*. Perispermic membrane. *h*. Parenchymatous tube of the micropyle. *i*. Chalaza. *k*. Funiculus.

Fig. 94. Sandstone cast of the interior of the testa of *Trigonocarpon olivæforme*, showing the rounded exterior of the nucleus with two of the narrow radial extensions of the endotestal cavity and the prolonged triangular terminal microplye.

Figs. 95–99. *Trigonocarpon olivæforme.* Five consecutive longitudinal sections, of which fig. 95 is the most peripheral, and fig. 97 the most central one.

a. Exotesta or sarcotesta. *b.* Hard endotesta. *c.* Lagenostomal cavity. *d.* Micropyle. *f.* Nucular membrane. *g.* Perispermic membrane often collapsed. All these figures are of the natural size.

Figs. 100–108. Series of transverse sections, of which fig. 100 is made through the lower part of the triangular micropyle, and fig. 108 near the chalazal base of the seed. The others are intermediate in the order of their numbers. The letters of reference are the same as in the longitudinal series.

Fig. 109. Transverse section of the cells of the endotesta in the neighbourhood of the chalaza. Enlarged 108 diameters.

Fig. 110. Transverse section of the sarcotesta immediately below the chalaza; enlarged 108 diameters. The central aperture is doubtless that through which a vascular bundle entered the seed.

Fig. 111. Vertical section of a portion of the testa. Enlarged 62 diameters.

a. Cells of sarcotesta. *b.* Endotesta.

Fig. 112. Transverse section of the same, from part of fig. 106.

Fig. 113. Upper extremity of the nucular membrane of fig. 96. Enlarged 15 diameters.

c. Lagenostome. *f.* Nucular membrane. *g.* Perispermic membrane.

Fig. 114. Similarly enlarged portion of fig. 97.

Fig. 115. Similarly enlarged portion of fig. 98.

Fig. 115 *a.* *Hexapterospermum Nöggerathi,* nat. size.

Fig. 115 *b.* *H. Nöggerathi,* enlarged 3 diameters.

Fig. 116. *Cardiocarpon anomalum?* Superficial aspect. Enlarged 11 diameters.

a. Exotesta. *b.* Endotesta or nucular membrane? *d.* Micropyle. *n.* Nucleus.

Fig. 117. Slightly oblique longitudinal section of fig. 116. Enlarged 15 diameters.

a. Exotesta. *b.* Endotesta or nucular membrane. *g.* Perispermic membrane. *i.* Chalaza. *k.* Funiculus. *n.* Nucleus.

Fig. 118. Vertical section of another example of *Cardiocarpon anomalum.* Enlarged 22 diameters.

a. Exotesta. *b.* Endotesta. *d.* Micropyle. *i.* Chalaza.

Fig. 119. Vertical section of another specimen of the same. Enlarged 22 diameters.

a. Exotesta. *b.* Endotesta. *d.* Micropyle. *i.* Chalaza.

Fig. 120. Parenchymatous cells of the perisperm of fig. 116. Enlarged 50 diameters.

Fig. 121. *Cardiocarpon compressum.* Approximate external form.

Fig. 122. *Cardiocarpon acutum* of Lindley and Hutton.

Fig. 123. Vertical section of the upper half of a seed of *Cardiocarpon compressum,* made on the plane of its greatest diameter. Enlarged 11 diameters.

Fig. 124. Transverse section of the same seed from the lower end of fig. 123.

Fig. 125. Transverse section of the base of the same seed passing through the chalaza.

Fig. 126. Longitudinal section of another specimen of *Cardiocarpon compressum* made vertically to the broad surface of fig. 121, and nearly at its centre.

a. Exotesta. *b*. Endotesta. *d*. Micropyle. *f*. Nucular membrane. *g*. Perispermic membrane. *n*. Nucleus.

Fig. 127. A vertical section of the same specimen made on one side of its centre along the line fig. 121, *x*.

Fig. 128. A vertical section made through the line of fig. 121 *y*.

Fig. 128*. Transverse section of *Cardiocarpon compressum* near its base.

Fig. 129. *Cardiocarpon Butterworthii*. External aspect. Natural size.

Fig. 130. *Cardiocarpon Butterworthii*. Enlarged 6 diameters.

Fig. 131. *Cardiocarpon Butterworthii*. Cells of the perisperm of a second and somewhat larger specimen. Enlarged 50 diameters.

Figs. 132, 133, 134. Three seeds from the Upper Coal-measures near Stonehouse, Lanarkshire. Enlarged 7 diameters. Apparently the *Cardiocarpum tenellum* of Dr. Dawson.

Fig. 135. Half of a seed of *Polypterospermum* from the uppermost Coal-measures of Ardwick. Natural size.

Figs. 136 & 137. The two halves, apparently, of the same seed, enlarged 4 diameters.

VIII. *On the Influence of Geological Changes on the Earth's Axis of Rotation.* By GEORGE H. DARWIN, *M.A., Fellow of Trinity College, Cambridge. Communicated by* Professor J. C. ADAMS.

Received October 13,—Read November 23, 1876.

THE subject of the fixity or mobility of the earth's axis of rotation in that body, and the possibility of variations in the obliquity of the ecliptic, have from time to time attracted the notice of mathematicians and geologists. The latter look anxiously for some grand cause capable of producing such an enormous effect as the glacial period. Impressed by the magnitude of the phenomenon, several geologists have postulated a change of many degrees in the obliquity of the ecliptic and a wide variability in the position of the poles on the earth; and this, again, they have sought to refer back to the upheaval and subsidence of continents.

Mr. JOHN EVANS, F.R.S., the late President of the Geological Society, in an address delivered to that Society, has recurred to this subject at considerable length. After describing a system of geological upheaval and subsidence, evidently designed to produce a maximum effect in shifting the polar axis, he asks:—"Would not such a modification of form bring the axis of figure about 15° or 20° south of the present, and on the meridian of Greenwich—that is to say, midway between Greenland and Spitzbergen? and would not, eventually, the axis of rotation correspond in position with the axis of figure?

"If the answer to these questions is in the affirmative, then I think it must be conceded that even minor elevations within the tropics would produce effects corresponding to their magnitude, and also that it is unsafe to assume that the geographical position of the poles has been persistent throughout all geological time."[*]

On the few occasions on which this subject has been referred to by mathematicians, the adequacy of geological changes to produce effects of such amount has been denied. Amongst others, the Astronomer Royal and Sir WILLIAM THOMSON have written briefly on the subject[†], but, as far as I know, the subject has not hitherto been treated at much length.

The following paper is an attempt to answer the questions raised by Mr. EVANS; but as I have devoted a section to the determination of the form of continent and sea which would produce a maximum effect in shifting the polar axis, I have not taken into consideration the configuration proposed by him.

The general plan of this paper is to discuss the following problems:—

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[*] Quart. Journ. Geol. Soc. 1876, xxxii. Proc. p. 108.

[†] In papers referred to below.

First. The precession and nutation of a body slowly changing its shape from internal causes, with especial reference to secular alterations in the obliquity of the ecliptic.

Second. The changes in the position of the earth's axis of symmetry, caused by any deformations of small amount.

Third. The modifications introduced by various suppositions as to the nature of the internal changes accompanying the deformations.

In making numerical application of the results of the previous discussions to the case of the earth, it has of course been necessary to betake one's self to geological evidence; but the vagueness of that evidence has precluded any great precision in the results.

In conclusion I must mention that, since this paper has been in manuscript, Sir WILLIAM THOMSON, in his Address to the Mathematical Section of the British Association at Glasgow, has expressed his opinion on this same subject. He there shortly states results in the main identical with mine, but without indicating how they were arrived at.

The great interest which this subject has recently been exciting both in England and America, coupled with the fact that several of my results are not referred to by Sir WILLIAM THOMSON, induces me to persist in offering my work to the Royal Society.

I. PRECESSION OF A SPHEROID SLOWLY CHANGING ITS SHAPE.

I begin the investigation by discussing the precession and nutation of an ellipsoid of revolution slowly and uniformly changing its shape. The changes are only supposed to continue for such a time, that the total changes in the principal moments of inertia are small compared to the difference between the greatest and least moments of inertia of the ellipsoid in its initial state.

For brevity, I speak of the ellipsoid as the earth; and shall omit some parts of the investigation, which are irrelevant to the problem under discussion.

The changes are supposed to proceed from internal causes, and to be any whatever; and in the application made they will be supposed to go on with a uniform velocity.

1. *The Equations of Motion.*

M. LIOUVILLE has given the equations of motion about a point of a body which is slowly changing its shape from internal causes[*]; these equations, he says, are only applicable to the case of the point being fixed or moving uniformly in a straight line. They may, however, be extended to the motion of the earth about its centre of inertia, because the centrifugal force due to the orbital motion and the unequal orbital motion will not add any thing to the moments of the impressed forces. These equations are, in fact, an extension of EULER's equations for the motion of a rigid body, which are ordinarily applied to the precessional problem. To make them intelligible I reproduce the following from Mr. ROUTH's 'Rigid Dynamics'[†], where the proof is given more succinctly than in the original:—

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[*] LIOUVILLE's Journ. Math. 2^{me} série, t. iii. 1858, p. 1.

[†] Page 150, edit. of 1860, but omitted in later editions. L, M, N are the couples of the impressed forces about the axes.

"Let x, y, z be the coordinates of any particle of mass m at the time t, referred to axes fixed in space. Then we have the equation of motion

$$\Sigma m\left(x\frac{d^2y}{dt^2}-y\frac{d^2x}{dt^2}\right)=\mathrm{N} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and two similar equations.

"Let

$$h_3=\Sigma m\left(x\frac{dy}{dt}-y\frac{dx}{dt}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

with similar expressions for h_1, h_2.

"Then the equation (1) becomes

$$\frac{dh_3}{dt}=\mathrm{N} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

"Let the motion be referred to three rectangular axes Ox', Oy', Oz' moving in any manner about the origin O. Let α, β, γ be the angles these three axes make with the fixed axis of z. Now h_3 is the sum of the products of the mass of each particle into twice the projection on the plane of xy of the area of the surface traced out by the radius vector of that particle drawn from the origin. Let h'_1, h'_2, h'_3 be the corresponding 'areas' described on the planes $y'z'$, $z'x'$, $x'y'$ respectively. Then by a known theorem proved in Geometry of Three Dimensions, the sum of the projections of h_1', h'_2, h'_3 on xy is equal to h_3;

$$\therefore\ h_3=h'_1\cos\alpha+h'_2\cos\beta+h'_3\cos\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

"Since the fixed axes are quite arbitrary, let them be taken so that the moving axes are passing through them at the time t. Then

$$h'_1=h_1,\quad h'_2=h_2,\quad h'_3=h_3;$$

and by the same reasoning, as in Arts. 114 and 115, we can deduce from equation (4) that

$$\frac{dh_3}{dt}=\frac{dh'_3}{dt}-h'_1\theta_2+h'_2\theta_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where θ_1, θ_2, θ_3 are the angular velocities of the axes with reference to themselves. Hence the equations of motion of the system become

$$\left.\begin{aligned}\frac{dh_1}{dt}-h_2\theta_3+h_3\theta_2&=\mathrm{L},\\ \frac{dh_2}{dt}-h_3\theta_1+h_1\theta_3&=\mathrm{M},\\ \frac{dh_3}{dt}-h_1\theta_2+h_2\theta_1&=\mathrm{N},\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

"These equations may be put under another form which is more convenient. Let x', y', z' be the coordinates of the particle m referred to the moving axes, and let

$$\mathrm{H}_3=\Sigma m\left(x'\frac{dy'}{dt}-y'\frac{dx'}{dt}\right).$$

"Since the fixed axes coincide with these at the time t, we have $x=x'$, $y=y'$, and by Art. 114,

$$\left.\begin{aligned}\frac{dx}{dt}&=\frac{dx'}{dt}+\theta_2 z-\theta_3 y\\ \frac{dy}{dt}&=\frac{dy'}{dt}+\theta_3 x-\theta_1 z\end{aligned}\right\};$$

$$\therefore h'_3=\mathrm{H}_3+\mathrm{C}\theta_3-\mathrm{E}\theta_1-\mathrm{D}\theta_2{}^*;$$

and by similar reasoning

$$h'_1=\mathrm{H}_1+\mathrm{A}\theta_1-\mathrm{F}\theta_2-\mathrm{E}\theta_3,$$

$$h'_2=\mathrm{H}_2+\mathrm{B}\theta_2-\mathrm{D}\theta_3-\mathrm{F}\theta_1.$$

"Hence the general equation of motion becomes

$$\frac{d}{dt}(\mathrm{C}\theta_3-\mathrm{E}\theta_1-\mathrm{D}\theta_2+\mathrm{H}_3)+\mathrm{F}(\theta_2^2-\theta_1^2)+(\mathrm{B}-\mathrm{A})\theta_1\theta_2+\mathrm{E}\theta_2\theta_3-\mathrm{D}\theta_1\theta_3+\theta_1\mathrm{H}_2-\theta_2\mathrm{H}_1=\mathrm{N} \quad . \quad (7)$$

and two similar equations.

"Let the moving axes be so chosen as to coincide with the principal axes at the time t. Then $\mathrm{D}=0$, $\mathrm{E}=0$, $\mathrm{F}=0$, and the equations become,"

$$\frac{d}{dt}(\lambda_1\theta_1+\mathrm{H}_1)-(\lambda_2-\lambda_3)\theta_2\theta_3+\theta_2\mathrm{H}_3-\theta_3\mathrm{H}_2=\mathrm{L},$$

and two similar equations; where λ_1, λ_2, λ_3 (replacing the A, B, C of Mr. ROUTH) are the three principal moments of inertia, and are functions of the time.

In order to apply these equations to the present problem, we must consider the meaning of the quantities θ_1, θ_2, θ_3. A system of particles may be made to pass from any one configuration to any other by means of the rotation of the system as a whole about any axis through any angle, and a subsequent displacement of every particle in a straight line to its ultimate position. Of all the axes and all the angles about and through which the preliminary rotation may be made, there is one such that the sum of the squares of the subsequent paths is a minimum. By analogy with the method of least squares this rotation may be said to be that which most nearly represents the passage of the system from one configuration to the other. If the two configurations differ by little from one another, and if the best representative rotation be such that the curvilinear path of any particle is large compared to its subsequent straight path, the system may be said to be rotating as a rigid body, and at the same time slowly changing its shape. Now this is the case we have to consider in a slow distortion of the earth.

Divide the time into a number of equal small intervals τ, and in the first interval let the earth be rigid, and let each pair of its principal axes rotate about the third (with angular velocities double those with which they actually rotate). At the end of that interval suppose that each pair has rotated about the third through angles $2\omega_1\tau$, $2\omega_2\tau$,

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[*] A, B, C, D, E, F are, as usual, the moments and products of inertia.

$2\omega_3\tau$. Then reduce the earth to rest, and during the next interval let the matter constituting the earth flow (with velocities double those with which it actually flows) so that the pairs of principal axes have, at the end of the interval, rotated with respect to the third ones through the angles $-2\alpha\tau$, $-2\beta\tau$, $-2\gamma\tau$. Lastly let $2\theta_1\tau$, $2\theta_2\tau$, $2\theta_3\tau$ be the rotations of each pair of axes about the third by which they could have been brought directly from their initial to their final positions in the time 2τ.

Therefore, by the principle of superposition of small motions,

$$\theta_1=\omega_1-\alpha,\ \theta_2=\omega_2-\beta,\ \theta_3=\omega_3-\gamma.$$

Now supposing these two processes to go on simultaneously with their actual velocities, instead of in alternate intervals of time with double velocities, it is clear that θ_1, θ_2, θ_3 are "the angular velocities of the axes with reference to themselves"; ω_1, ω_2, ω_3 are the component angular velocities of the earth considered as a rigid body; and $-\alpha$, $-\beta$, $-\gamma$ are the component angular velocities of the principal axes relatively to the earth, arising from the supposing continuous distortion of that body.

With respect to the other quantities involved in the equations of motion:—

Let C, A be the principal moments of inertia of the earth initially when t is zero; and at any time t, let

$$\lambda_1=\mathrm{A}+\mathrm{a}t,\ \lambda_2=\mathrm{A}+\mathrm{b}t,\ \lambda_3=\mathrm{C}+\mathrm{c}t.$$

We here suppose that the changes in the earth are so slow that terms depending on higher powers of t may be neglected.

Lastly the quantities H_1, H_2, H_3 are respectively twice the areas conserved on the planes of $\theta_2\theta_3$, $\theta_3\theta_1$, $\theta_1\theta_2$ by the motion of the earth relative to these axes. If the earth were rigid, they would all be zero, because there would be no motion relative to the principal axes: thus ω_1, ω_2, ω_3 do not enter into these quantities. Now the motion which *does* take place may be analyzed into two parts. Divide the time into a number of equal small elements τ, and in the first of them let the matter constituting the earth flow (with a velocity double that with which it actually flows); this motion will conserve double-areas on the planes of $\theta_2\theta_3$, $\theta_3\theta_1$, $\theta_1\theta_2$, which we may call $2\mathfrak{H}_1\tau$, $2\mathfrak{H}_2\tau$, $2\mathfrak{H}_3\tau$. In the next interval of time let each pair of axes rotate round the third (with angular velocities double those with which they actually rotate), so that at the end of the interval they have turned through the angles $-2\alpha\tau$, $-2\beta\tau$, $-2\gamma\tau$. Now since during this second interval the axes have rotated in a negative direction through the solid, therefore the solid has rotated in a positive direction with reference to the axes. Remembering then that λ_1, λ_2, λ_3 are the principal moments of inertia, the double-areas conserved on the three planes in this second interval are $2\lambda_1\alpha\tau$, $2\lambda_2\beta\tau$, $2\lambda_3\gamma\tau$. Hence if $2\mathrm{H}_1\tau$, $2\mathrm{H}_2\tau$, $2\mathrm{H}_3\tau$ be the double areas conserved in this double interval of time, we have $2\mathrm{H}_1\tau=2\mathfrak{H}_1\tau+2\lambda_1\alpha\tau$, $2\mathrm{H}_2\tau=2\mathfrak{H}_2\tau+2\lambda_2\beta\tau$, $2\mathrm{H}_3\tau=2\mathfrak{H}_3\tau+2\lambda_3\gamma\tau$.

Therefore if we now suppose the two processes to go on simultaneously with their actual velocities, instead of in alternate elements of time with double velocities, and if we substitute for λ_1, λ_2, λ_3 their values in terms of A, a, t, &c., we get

$$H_1=(A+at)\,\alpha+\mathfrak{h}_1,\ \ H_2=(A+bt)\beta+\mathfrak{h}_2,\ \ H_3=(C+ct)\gamma+\mathfrak{h}_3,$$

where $\mathfrak{h}_1$, $\mathfrak{h}_2$, $\mathfrak{h}_3$, denote those parts of the double areas conserved, which depend only on the internal motions accompanying the change of shape.

Then, if the changes proceed with uniform velocity, a, α, $\mathfrak{h}$, &c. are all constant.

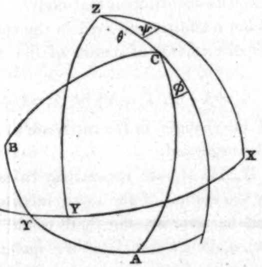
Corresponding also to the equations of motion are the geometrical equations

$$\frac{d\theta}{dt}=\theta_2\cos\phi+\theta_1\sin\phi\,;$$

$$\frac{d\psi}{dt}\sin\theta=-\theta_1\cos\phi+\theta_2\sin\phi\,;$$

$$\frac{d\phi}{dt}+\frac{d\psi}{dt}\cos\theta=\theta_3.$$

Fig. 1.



In figure 1, A, B, C are the axes, about which the moments of inertia are $\lambda_1, \lambda_2, \lambda_3$; XY is the ecliptic; and the meaning of the other symbols is sufficiently indicated.

Substituting, then, for the various symbols in the original equations of motion, it will be found that

$$A\frac{d\omega_1}{dt}-(A'-C)\omega_2\omega_3=L-t\left\{a\frac{d\omega_1}{dt}-(b-c)\omega_2\omega_3+b\gamma\omega_2-c\beta\omega_3\right\}-a\omega_1-\{A'\gamma+\mathfrak{h}_3\}\omega_2$$
$$+\{C\beta+\mathfrak{h}_2\}\omega_3+\beta\mathfrak{h}_3-\gamma\mathfrak{h}_2,$$

and two similar equations*.

Now the terms on the right-hand side are always very small compared to $A\frac{d\omega_1}{dt}$, because the time will not run on until they have become large; hence approximate values may be substituted therein.

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* The A is written A′ in two places, where it may be taken to stand for B; and then the other equations may be found by cyclic changes of letters and suffixes.

Let the angular velocity of rotation of the earth be $-n$, and let $\Pi \operatorname{cosec} \theta$ be the precession of the equinoxes; then in the small terms the following substitutions may be made:—

$$\phi=-(nt+e),\ \omega_1=-\Pi\cos(nt+e),\ \omega_2=-\Pi\sin(nt+e),\ \omega_3=-n,$$

and the e may be omitted for brevity.

Further, N (depending on the attractions of the sun and moon) is very small; and a consideration of the third equation of motion shows that, when integrated, it leads to $\omega_3=-n+$ terms, which are very small during the limited period under consideration. And if these terms were substituted on the left-hand side of the two former equations, they would be still further diluted by multiplication by the small quantities $\frac{C-A}{A}$ and Π. Hence the third equation may be neglected, and in the two former equations $-n$ may be substituted for ω_3.

Making these substitutions, then, and writing μ for $\frac{C-A}{A}n$, the equations become

$$\frac{d\omega_1}{dt}-\mu\omega_2=\frac{L}{A}-\frac{\Pi n}{A}\left\{\mathrm{a}-\mathrm{b}+\mathrm{c}-\frac{\mathrm{b}\gamma}{n}\right\}t\sin nt+\frac{\Pi\mathrm{a}}{A}\cos nt+\Pi\left\{\gamma+\frac{\mathfrak{h}_3}{A}\right\}\sin nt$$
$$-\beta\left\{\frac{(C+ct)n-\mathfrak{h}_3}{A}\right\}-\frac{\mathfrak{h}_2}{A}(n+\gamma);$$

$$\frac{d\omega_2}{dt}+\mu\omega_1=\frac{M}{A}-\frac{\Pi n}{A}\left\{-\mathrm{a}+\mathrm{b}+\mathrm{c}-\frac{\mathrm{a}\gamma}{n}\right\}t\cos nt+\frac{\Pi\mathrm{b}}{A}\sin nt-\Pi\left(\gamma+\frac{\mathfrak{h}_3}{A}\right)\cos nt$$
$$+\alpha\left\{\frac{(C+ct)n-\mathfrak{h}_3}{A}\right\}+\frac{\mathfrak{h}_1}{A}(n+\gamma).$$

Then neglecting $\frac{\gamma}{n}$ compared to unity, putting $C+ct=A$ in the small terms, and only retaining the more important terms,

$$\frac{d\omega_1}{dt}-\mu\omega_2=\frac{L}{A}-\frac{\Pi n}{A}\{\mathrm{a}-\mathrm{b}+\mathrm{c}\}t\sin nt+\frac{\Pi\mathrm{a}}{A}\cos nt+\Pi\left\{\gamma+\frac{\mathfrak{h}_3}{A}\right\}\sin nt-n\beta,$$
$$\frac{d\omega_2}{dt}+\mu\omega_1=\frac{M}{A}+\frac{\Pi n}{A}\{-\mathrm{a}+\mathrm{b}+\mathrm{c}\}t\cos nt+\frac{\Pi\mathrm{b}}{A}\sin nt-\Pi\left\{\gamma+\frac{\mathfrak{h}_3}{A}\right\}\cos nt+n\alpha^{*}.$$

These are the required equations of motion, and in integrating them they may be treated as linear.

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[*] If we wish to treat a, α, $\mathfrak{h}_1$, &c. as variable, we have only to add to the right-hand sides of these equations $\frac{\Pi}{A}t\cos nt\frac{d\mathrm{a}}{dt}-\frac{1}{A}\frac{d\mathfrak{h}_1}{dt}$, and $\frac{\Pi}{A}t\sin nt\frac{d\mathrm{b}}{dt}-\frac{1}{A}\frac{d\mathfrak{h}_2}{dt}$ respectively. If we put $L=M=0$ and neglect $\mathfrak{h}_1$, $\mathfrak{h}_2$, $\mathfrak{h}_3$, these equations will be found to be identical with the equations (2) given by Sir W. THOMSON in App. C. I had not noticed until it was pointed out by him, how nearly applicable my equations were to the case of varying velocities of distortion.—April 26, 1877.

2. *Inequalities independent of the Impressed Forces.*

First, then, suppose that $L=M=0$.

Integrate the equations, and neglect μ compared with n, and we have

$$\omega_1=\frac{\Pi}{A}(\mathrm{a}-\mathrm{b}+\mathrm{c})\,t\cos nt+\frac{\Pi}{An}(\mathrm{b}-\mathrm{c})\sin nt-\frac{\Pi}{n}\left(\gamma+\frac{\mathfrak{P}_3}{A}\right)\cos nt$$

$$+\frac{n}{\mu}\left(\alpha+\frac{\mathfrak{P}_1}{A}\right)+F\cos\mu t-G\sin\mu t,$$

$$\omega_2=\frac{\Pi}{A}(-\mathrm{a}+\mathrm{b}+\mathrm{c})\,t\sin nt+\frac{\Pi}{An}(\mathrm{c}-\mathrm{a})\cos nt-\frac{\Pi}{n}\left(\gamma+\frac{\mathfrak{P}_3}{A}\right)\sin nt$$

$$+\frac{n}{\mu}\left(\beta+\frac{\mathfrak{P}_2}{A}\right)-F\sin\mu t-G\cos\mu t.$$

The last two terms in ω_1 and ω_2 represent the complementary function; and the values of F and G must be determined from the initial conditions.

Now it will be shown later that α, β, γ are comparable with $\frac{\mathrm{a}}{C-A}, \frac{\mathrm{b}}{C-A}, \frac{\mathrm{c}}{C-A}$; hence the terms in the second lines are much more important than those in the first. Thus in determining the values of F and G we may neglect the first lines.

Now, initially, the instantaneous axis coincides with the axis of greatest moment of inertia; so that when $t=0$, $\omega_1=\omega_2=0$, and therefore

$$F=-\frac{n}{\mu}\left(\alpha+\frac{\mathfrak{P}_1}{A}\right),\ G=\frac{n}{\mu}\left(\beta+\frac{\mathfrak{P}_2}{A}\right).$$

The terms in F and G represent an inequality of 306 days period.

3. *The Inequality of* 306-*Days Period**.

I have worked out the value of $\mathfrak{P}_1$ and $\mathfrak{P}_2$ in two supposed cases of elevation, under certain suppositions as to the nature of the internal movements of the earth. In one of them I found $\frac{\mathfrak{P}_2}{\beta \mathrm{A}}=\frac{1}{141}$, and $\mathfrak{P}_1=0$; and in the other $\mathfrak{P}_1=\mathfrak{P}_2=0$. In order not to interrupt the thread of the argument, the calculation is given in Appendix A; it will also be more intelligible after the latter part of this paper has been read. In the general case the same kind of proportion will subsist between $\mathfrak{P}_1$ and $\mathrm{A}\alpha$, $\mathfrak{P}_2$ and $\mathrm{A}\beta$, and we may therefore, without serious error, neglect the former compared with the latter.

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* I have thought it necessary to discuss this inequality fully, both on account of its intrinsic interest, and because it has been referred to by the Astronomer Royal and Sir WILLIAM THOMSON.

The former says ('Athenæum,' Sept. 22, 1860):—"Now, let us suppose the earth not absolutely rigid, but that there is susceptibility to change of form, either from that degree of yielding or fracture to which most solid substances are liable, or from the hydrostatic pressure of internal fluid. This, as I conceive, puts an end to all supposition of change of axis. The first day's whirl would again make the axis of rotation to be a principal axis, and the position of the axis would then be permanent."

But Sir GEORGE AIRY is here speaking of the effect of the elevation of a mountain mass in about latitude 45°, by something like a gaseous explosion. This supposition is not at all in accordance with the belief of geologists, whereas a gradual elevation is so.

Sir W. THOMSON, on the other hand, says (Trans. Geol. Soc. Glasgow, 1874, vol. xiv. p. 312):—"In the present condition of the earth, any change in the axis of rotation could not be permanent, because the instantaneous axis would travel round the principal axis of the solid in a period of 296 days. . . . . In very early geologic ages, if we suppose the earth to have been plastic, the yielding of the surface might have made the new axis a principal axis. But certain it is that the earth at present is so rigid that no such change is possible." And he adds that practical rigidity has prevailed throughout geologic history.

Thus, as far as concerns the present inequality,

$$\omega_1=\frac{n\alpha}{\mu}(1-\cos\mu t)-\frac{n\beta}{\mu}\sin\mu t,$$

$$\omega_2=\frac{n\alpha}{\mu}\sin\mu t+\frac{n\beta}{\mu}(1-\cos\mu t).$$

On account of this inequality the greatest angular distance (in radians) of the instantaneous axis from the pole is $\frac{2\sqrt{\alpha^2+\beta^2}}{\mu}$. It will appear from the latter part of this paper that, if the elevation of a large continent proceeds at the rate of two feet in a century, $\sqrt{\alpha^2+\beta^2}$ may be about $\frac{1}{100}''$ per annum, and μ is 360° in 306 days; whence it follows that the greatest angle made by the instantaneous axis with the axis of figure is comparable with $\frac{1}{376}''$, a quantity beyond the power of observation. On the score of these terms the instantaneous axis will therefore remain sensibly coincident with the axis of figure.

They will, moreover, produce no secular alteration on the obliquity of the ecliptic, nor in the precession, because they will appear as periodic in $\frac{d\theta}{dt}$ and $\frac{d\psi}{dt}\sin\theta$, with arguments n and $n\pm\mu$.

Now although this inequality is so small, it nevertheless is of interest.

If we map, on a tangent plane to the earth at its initial pole, the relative motion of the instantaneous axis and the pole of figure, we get, as the equation to the curve,

$$x=\frac{\alpha}{\mu}(1-\cos\mu t)-\frac{\beta}{\mu}\sin\mu t,$$

$$y=\frac{\alpha}{\mu}\sin\mu t+\frac{\beta}{\mu}(1-\cos\mu t).$$

If t be eliminated from these equations, we get

$$\left(x-\frac{\alpha}{\mu}\right)^2+\left(y+\frac{\beta}{\mu}\right)^2=\frac{\alpha^2+\beta^2}{\mu^2}.$$

Thus the relative motion is a circle, passing through the origin, and touching a line inclined to the axis of y at an angle arc $\tan\frac{\alpha}{\beta}$. Therefore the instantaneous axis describes a circle passing through the pole of figure every 306th day; and this circle touches the meridian, along which the axis of figure is travelling with uniform velocity, in consequence of the geological deformation of the earth.

The motion of the instantaneous axis in the earth is a prolate cycloid.

4. *Adjustments to a Form of Equilibrium.*

If the earth were a viscous fluid there is no doubt but that the pole of figure would tend to displace itself towards the instantaneous axis, whose mean position would be the centre of the circle above referred to.

But Sir WILLIAM THOMSON has shown* that the earth is sensibly rigid; and in any case the earth is not a viscous fluid, properly so called, although it may be slightly plastic.

M. TRESCA has shown that all solids are plastic under sufficiently great stresses, but that, until a certain magnitude of stress is reached, the solid refuses to flow†. Now in the case of a very small inequality like this, the stresses introduced by the want of coincidence of the instantaneous axis with the axis of figure are very small, even when at their maximum; and every 306th day they are zero. It seems, therefore, extremely improbable that the stresses can be great enough to bring the earth into what M. TRESCA calls the state of fluidity; and therefore it is unlikely that there can be any adaptation of the earth's form to a new form of equilibrium in consequence thereof.

In all the other inequalities introduced, whether arising from the first three terms above given in ω_1 and ω_2, or arising from the impressed forces, to be treated hereafter, the centre of the positions of the instantaneous axis is coincident with the pole of figure, and therefore there can hardly be any adaptation of figure eccentric to the axis of greatest moment to balance the stresses introduced by centrifugal force.

It would appear probable that, whilst a geological change is taking place, the earth is practically rigid for long periods. But as the earth comes to depart more and more from a form of equilibrium, the stresses due to the mutual gravitation of the parts, and to the rotation, increase gradually, until they are sufficiently great to cause the solid matter to flow. A rough kind of adjustment to a form of equilibrium would then take place. The existence of continents, however, shows that this adjustment does not take place by the subsidence of the upheaved part; and as this adaptation of form would be produced by an entirely different cause from that to which the upheaval was due, that upheaval would probably persist independently of the approximate adoption of a new form of equilibrium by the earth.

M. TRESCA's experiments on the punching of metals would lead one to believe that the change would take place somewhat suddenly, and would, in fact, be by an earthquake, or a succession of earthquakes. On each of these occasions the tendency would be to adjust the form to one of equilibrium about the instantaneous axis. Now the principal axis λ_3 has (in consequence of the postulated deformation) been travelling along the meridian in longitude $\pi + \arc \tan \frac{\alpha}{\beta}$, measured from the plane containing λ_1 and λ_3.

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* In his Address to the British Association, 1876, he states that the argument derived from precession (THOMSON and TAIT's Nat. Phil. p. 691) is fallacious; he adduces, however, a number of cogent arguments on this point.

† "Sur l'écoulement des Corps Solides," Mém. des Sav. tom. xviii.

The earthquake will take place when, to the stresses due to mutual gravitation, are superadded the maximum stresses due to centrifugal force; that is to say, when the instantaneous axis is at its greatest distance from λ_3, the axis of greatest moment of inertia. At the instant of the earthquake the principal axis will be moved towards the position of the instantaneous axis. And as the circle described by the instantaneous axis touches the meridian of displacement of the principal axis, therefore the principal axis will be carried by the adjustment towards the centre of the circle described by the instantaneous axis, and therefore perpendicular to the meridian of displacement.

Thus, if the adjusting earthquakes take place at long intervals, the motion of the principal axis will not deviate sensibly from continuing along the meridian, along which it would travel in consequence merely of the geological deformation. If, however, the adjustments are frequent, the path of λ_3 will diverge sensibly from the meridian along which it started. If the readjustments become infinitely frequent and infinitely small, there is a continuous flow of the matter of the earth, which is always seeking to bring back the earth's figure to one of equilibrium, from which figure it is also supposed to be continuously departing under the action of internal forces. In this state the earth may be considered as formed of a stiff viscous fluid.

According to these ideas, at each adjustment λ_1, λ_2, λ_3 will be suddenly reduced to nearly their primitive values, A, A, C; but α, β, γ depend on the rate of accession and diminution of matter at various parts of the earth, and remain constant. The only effect, then, is that each adjusting earthquake must be taken as a new epoch.

As far as I can see, it seems quite possible that the earth may be sensibly rigid to the tidally deforming influences of the sun and moon, and yet may bring itself back from any considerable departure from a form of equilibrium to approximately that form. It therefore seems worth while to consider the case of the adjustments being continuous, whilst the deformation is also continuous.

5. *Adjustments to the Form of Equilibrium continuous.*

I therefore propose to consider geometrically, but not dynamically, the paths of the instantaneous axis, and of the principal axis, where the earth is viscous and continuously deformed by internal forces. It is supposed that the velocities of flow of the matter of the earth are so small that inertia may be neglected, and that the displacements are so small that the principle of the superposition of small motions is applicable.

As before the paths of the instantaneous and principal axes may be mapped on a tangent plane to the spheroid, at the extremity of the primitive pole, the mean radius of the spheroid being taken as unity.

In consequence of the continuous deformation, the principal axis travels with a linear velocity (on the map) $-\sqrt{\alpha^2+\beta^2}$ along the meridian of longitude arc tan $\frac{\alpha}{\beta}$. Take this meridian as axis of x, and measure y, so that the angular velocity μ is from x towards y, and call $\sqrt{\alpha^2+\beta^2}$, u.

Then the principal axis λ_3 moves along the axis of x with a uniform linear velocity $-u$, and, from dynamical principles, the instantaneous axis I moves round the instantaneous position of λ_3 with a uniform angular velocity μ.

But because of the earth's viscosity, λ_3 always tends to approach I. The stresses introduced in the earth by the want of coincidence of λ_3 with I vary as λ_3I. Also the amount of flow of a viscous fluid, in a small interval of time, varies jointly as that interval and the stress. Hence the linear velocity (on the map), with which λ_3 approaches I, varies as λ_3I (equal to r suppose). Let this velocity be νr, where ν depends on the viscosity of the earth, diminishing as the viscosity increases.

Thus the principal axis describes a sort of curve of pursuit on the map; it is animated with a constant velocity $-u$ parallel to x, and with a velocity νr towards I, which rotates round it with a uniform angular velocity μ.

The motion of I, relative to λ_3, is that of a point moving with a constant velocity u parallel to x, rotating round a fixed point with a constant angular velocity μ, and moving towards that point with a velocity νr.

Let ξ, η be the relative coordinates of I with respect to λ_3, and x, y the coordinates of λ_3. Then the differential equations which give the above motions are:—

$$\frac{d\xi}{dt}=u-\nu\xi-\mu\eta, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{d\eta}{dt}=-\nu\eta+\mu\xi, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\frac{dx}{dt}=-u+\nu\xi, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$\frac{dy}{dt}=\nu\eta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

If (1) and (2) be integrated, and the constants determined so that, when $t=0$, $\xi=\eta=0$ (which expresses that initially λ_3 and I are coincident), it will be found that

$$\xi=\frac{u}{\nu^2+\mu^2}\left\{\nu(1-e^{-\nu t}\cos\mu t)+\mu e^{-\nu t}\sin\mu t\right\},$$

$$\eta=\frac{u}{\nu^2+\mu^2}\left\{\mu(1-e^{-\nu t}\cos\mu t)-\nu e^{-\nu t}\sin\mu t\right\}.$$

These give the path of I relative to λ_3. It may be seen to be a spiral curve diminishing with more or less rapidity, according as the earth is less or more viscous. If $\nu=0$, it becomes the circle, found above from the dynamical equations.

Substitute in (3) and (4) for ξ and η; integrate, and determine the constants, so that when $t=0$, $x=y=0$. It will then be found that

$$x=-\frac{u}{\nu^2+\mu^2}\left[\nu(\nu^2-\mu^2)+\mu^2t+\frac{\nu}{\nu^2+\mu^2}\{-(\nu^2-\mu^2)e^{-\nu t}\cos\mu t+2\mu\nu e^{-\nu t}\sin\mu t\}\right],$$

$$y=\frac{u}{\nu^2+\mu^2}\left[-2\mu\nu^2+\mu\nu t+\frac{\nu}{\nu^2+\mu^2}\{2\mu\nu e^{-\nu t}\cos\mu t+(\nu^2-\mu^2)e^{-\nu t}\sin\mu t\}\right].$$

These give the path of λ_3 on the map. It may be seen to be a cycloidal curve, in which the radius of the rolling circle diminishes with more or less rapidity, according as the earth is less or more viscous.

After some time $e^{-\nu t}$ becomes very small, and the motion is steady; and then $\xi=\frac{u\nu}{\mu^2+\nu^2}$, $\eta=\frac{u\mu}{\nu^2+\mu^2}$, or I is fixed, relatively to λ_3, at a distance $\frac{u}{\sqrt{\nu^2+\mu^2}}$ from it, and on the meridian, measured from the axis of x, in longitude arc tan $\frac{\mu}{\nu}$. This point is the centre of the above-mentioned spiral curve.

If ν be very small (or the earth nearly rigid) this meridian differs by little from the axis of y. But it may be that ν is so small that $e^{-\nu t}$ has not time to become insensible before the geological changes cease. This case corresponds very nearly to the hypothesis, in the last section, of adjusting earthquakes.

If the earth be very mobile, or ν large, $\xi=\frac{u}{\nu}$, $\eta=0$.

Again, with respect to the path of λ_3, when the motion has become steady,

$$x=-\frac{u\nu(\nu^2-\mu^2)}{\nu^2+\mu^2}-\frac{\mu^2u}{\nu^2+\mu^2}t,$$

$$y=-\frac{2\mu u\nu^2}{\nu^2+\mu^2}+\frac{\mu u\nu}{\nu^2+\mu^2}t,$$

and eliminating t, $\nu x+\mu y=-u\nu^2$.

That is to say, when the motion is steady, λ_3 moves parallel to meridian longitude $\pi-$arc tan $\frac{\nu}{\mu}$, and distant from it $\frac{u\nu^2}{\sqrt{\nu^2+\mu^2}}$ on the negative side. This straight line is the degraded form of the above-mentioned cycloidal curve.

If the earth is nearly rigid this path does not differ sensibly from the axis of x; if very mobile, it is nearly perpendicular to the axis of x, and a long way from the origin. In this last case the solution becomes nugatory, except as showing that the very small inequality of 306 days would be capable of disturbing and quite altering the path of the principal axis, as arising merely from geological changes on the surface of the earth.

In the case contemplated by the Astronomer Royal, where the elevation is explosive, u must be put equal to zero, and the constants of integration so determined, that when $t=0$, $\xi=$R suppose, and $\eta=x=y=0$. It will then be found that when the agitation has subsided, $x=\mathrm{R}\frac{\nu^2}{\mu^2}$, $y=\mathrm{R}\frac{\nu}{\mu}$, or the pole of figure will have taken up a position on one side of the meridian, along which it was initially propelled by the explosion.

It thus seems probable that during the consolidation of the earth there was a great instability in the position of the principal axis, and therefore also of the axis of rotation which followed it.

6. *Secular alteration in the obliquity of the Ecliptic, resulting from terms independent of the Impressed Forces.*

To return to the main line of the inquiry:—If the values of ω_1 and ω_2, found in sec. 2, be substituted in the geometrical equations for $\frac{d\theta}{dt}$ and $\frac{d\psi}{dt}\sin\theta$ (see sec. 1), a number of periodic terms will arise, and these terms have diurnal and semidiurnal periods, but their amplitudes are so small that they have no practical interest.

The only thing which concerns us is to inquire whether there can be any secular change in the obliquity of the ecliptic.

Select, then, only terms in $\sin nt$ in ω_1, and in $\cos nt$ in ω_2, and substitute in the geometrical equation $\frac{d\theta}{dt}=-\omega_1\sin nt+\omega_2\cos nt$, and reject periodic terms. It will then be found that

$$\frac{d\theta}{dt}=-\frac{\Pi}{2\mathrm{A}n}(\mathrm{a}+\mathrm{b}-2\mathrm{c}).$$

7. *Terms dependent on the Impressed Forces.*

It now remains to consider the effect of the impressed forces on the precession and obliquity of the ecliptic.

The equations of motion are reduced to

$$\frac{d\omega_1}{dt}+\frac{\mathrm{C}-\mathrm{A}}{\mathrm{A}}\omega_3\omega_2=\frac{\mathrm{L}}{\mathrm{A}},$$

$$\frac{d\omega_2}{dt}-\frac{\mathrm{C}-\mathrm{A}}{\mathrm{A}}\omega_3\omega_1=\frac{\mathrm{M}}{\mathrm{A}},$$

$$\frac{d\omega_3}{dt}=\frac{\mathrm{N}}{\mathrm{A}}.$$

If we write for L, M, N, L+δL, M+δM, δN, and indicate by L and M the couples caused by the attractions of the sun and moon on the protuberant parts of the earth before it has begun to change its shape, then L and M only cause the ordinary precession and nutations. For the present problem it is therefore only necessary to consider the effects of δL, δM, δN, which arise from the change of shape of the earth.

It follows, from the same arguments that were used in sec. 1, that the change in the earth's angular velocity of rotation due to δN will only have a very small effect on ω_1 and ω_2; so that, as far as is now important, ω_3 may be put equal to $-n$ in the first two equations, which may then be written

$$\frac{d\omega_1}{dt}-\mu\omega_2=\frac{\delta L}{A},$$

$$\frac{d\omega_2}{dt}+\mu\omega_1=\frac{\delta M}{A}.$$

Now δL and δM are the changes in L and M, when $A+at$, $A+bt$, $C+ct$ are written for A, A, and C respectively. If δL, δM be thus formed, and the equations integrated, it will be found that the principal terms, arising from the sun's attraction, are nine both in $\frac{d\theta}{dt}$ and $\frac{d\psi}{dt}\sin\theta$; the same number of terms arise in the precession and nutation with respect to the plane of the lunar orbit, and these would have to be referred to the ecliptic. Sixteen out of the eighteen terms represent, however, only very small nutations, and the only terms of any interest are those which give rise to a secular change in the obliquity of the ecliptic. These terms may be picked out without reproducing the long calculation above referred to, for they arise entirely out of the constant couple acting about the equinoctial line, which gives rise to the uniform precession.

Now this constant couple is $C\Pi n$; whence $L=C\Pi n\sin nt$, $M=-C\Pi n\cos nt$. And since Π involves $\frac{C-A}{C}$, therefore

$$\delta L=-C\Pi n\frac{b-c}{C-A}t\sin nt,\quad \delta M=-C\Pi n\frac{c-a}{C-A}t\cos nt.$$

If these be substituted in the equations of motion and the equations integrated, and only terms in $\sin nt$ in ω_1, and those in $\cos nt$ in ω_2, be retained, we get

$$\omega_1=-\frac{\Pi}{n}\,\frac{b-c}{C-A}\sin nt,\quad \omega_2=-\frac{\Pi}{n}\,\frac{c-a}{C-A}\cos nt.$$

Substituting in the geometrical equation $\frac{d\theta}{dt}=-\omega_1\sin nt+\omega_2\cos nt$, and rejecting periodic terms,

$$\frac{d\theta}{dt}=\frac{\Pi}{2n}\,\frac{a+b-2c}{C-A}.$$

8. *General result with respect to the Obliquity of the Ecliptic.*

It was found in sec. 6 that the secular rate of change of θ, as due to the internal changes in the earth, was $-\frac{\Pi}{2n}\cdot\frac{a+b-2c}{A}$. Since $C-A$ is small compared to A, this term is small compared with the term found at the end of sec. 7. Hence, finally, taking all the terms together, we get the approximate result,

$$\frac{d\theta}{dt}=\frac{\Pi}{2n}\cdot\frac{a+b-2c}{C-A},$$

and for small changes in the obliquity, insufficient to materially affect Π,

$$\theta=i+\frac{\Pi}{2n}\cdot\frac{a+b-2c}{C-A}t.$$

This equation has been obtained on the supposition that the change in the earth's form never becomes so great that at, bt, ct exceed small fractions of C—A; a condition which is satisfied in the case of such geological changes as those of which we have any cognizance at present.

It will appear from a comparison with results given hereafter, that $\frac{\mathrm{a}+\mathrm{b}-2\mathrm{c}}{C-A}t$ cannot ever exceed two or three degrees; and since $\frac{\Pi}{2n}$ is a very small fraction, it follows that *the obliquity of the ecliptic must have remained sensibly constant throughout geological history*[*]. *Also the instantaneous axis of rotation must always have remained sensibly coincident with the principal axis of figure, however the latter may have wandered in the earth's body.*

It has hitherto been assumed that the change of form and the angular velocities of the principal axes in the earth's body are uniform. But the preceding investigation shows clearly that no material change would be brought about by supposing the changes to proceed with varying velocities. This being so, dynamical considerations may be dismissed henceforth; and accordingly the next part of this paper will be devoted to the kinematical question, as to the change in position of the earth's axis of figure as due to geological changes.

The various assumptions made above will incidentally be justified in the course of the work.

For some remarks of Sir WILLIAM THOMSON on this part of the paper see Appendix C.

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[*] During the Glacial Period there must have been heavy ice-caps on one or both poles of the earth. The above equation will give the disturbance of the obliquity of the ecliptic produced thereby.

I will take what I believe is the most extreme view held by any geologist. Mr. BELT is of opinion that an enormous ice-sheet, which was thickest in about lat. 70° N. and S., descended from both poles down to lat. 45°; the amount of ice was so great that the sea stood some 2000 feet lower than now throughout the unfrozen regions between lat. 45° N. and S.

Suppose that the whole of this equatorial region was sea, and that the water contained in 2000 feet of depth of this sea was gradually piled on the polar regions in the form of ice. Then the effect in diminishing C and increasing A cannot be so great as if the whole of this mass were subtracted actually from the equator and piled actually on the poles. This latter supposition will then give a superior limit to the amount of alteration in the obliquity of the ecliptic. I have calculated this alteration by means of the above formula, taking the numerical data used later in this paper, and taking the specific gravity of water to that of surface-rock as 4 to 11. I find, then, that the superior limit to the increase of the obliquity of the ecliptic would be ·00045"; that is to say, the position of the arctic circle cannot have been shifted so much as half an inch. And this is an accumulated effect, and the matter is distributed in the most favourable manner possible.

In this case the amount of matter displaced is enormous, and is placed in the most favourable position for affecting the obliquity; hence, *à fortiori*, geological changes in the earth cannot have sensibly affected the obliquity.

But although this equation leads to no startling results in the geological history of the earth, I hope to show in a future paper that it may have some bearing on the very remote history of the earth and of the other planets (see a paper "On a Suggested Explanation of the Obliquity of Planets to their Orbits," Phil. Mag. March 1877).—[In consequence of a mistake in the work it was erroneously stated in the abstract of this paper in the 'Proceedings' that the change in the position of the arctic circles might amount to 3 inches, instead of to half an inch.—*Added* August 18, 1877.]

II. THE PRINCIPAL AXES OF THE EARTH.

9. *Preliminary Assumptions.*

It is assumed at first that, in consequence of some internal causes, the earth is undergoing a deformation, but that there is no disturbance of the strata of equal density, and that there is no local dilatation or contraction in any part of the body. The cases at present excluded will be considered later.

The result of this assumption is, that the volume of the body remains constant, and that the parts elevated or depressed above or below the mean surface of the ellipsoid have the same density as the rest of the surface. Such changes of form must, of course, be produced by a very small flow of the solid matter of the earth. Since the whole volume remains the same, this hypothesis may be conveniently called that of incompressibility; although, if the matter of the earth flowed quite incompressibly, there would be some slight dislocation of the strata of equal density.

It is immaterial for the present purpose what may be the forces which produce, and the nature of, this internal flow; but it was assumed in the dynamical investigation that the forces were internal, and that the flow proceeded with uniform velocity.

After deformation the body may be considered as composed of the original ellipsoid, together with a superposed layer of matter, which is positive in some parts and negative in others. The condition of constancy of volume necessitates that the total mass of this layer should be zero. If we take axes with the origin at the centre of the ellipsoid and symmetrical thereto, and let $h\,\mathrm{F}(\theta, \phi)$ represent the depth of the layer at the point θ, ϕ, the condition of incompressibility is expressed by the integral of $\mathrm{F}(\theta, \phi)$ over the surface of the ellipsoid being zero. Then by varying h, elevations and depressions of various magnitudes may be represented.

10. *Moments and Products of Inertia after Deformation.*

Before the deformation:—

Let A, C be the principal moments of inertia of the earth; a, b its semiaxes; M its mass; $\mathfrak{D}$ its mean density; ϱ its surface density; and c its mean radius, so that $3c=2a+b$; and let the earth's centre of inertia be at the origin.

After the deformation:—

Let a, b, c, D, E, F be the moments and products of inertia of the above ideal shell of matter about the axes; $\bar{x}, \bar{y}, \bar{z}$ the coordinates of the earth's centre of inertia.

Then, since the ellipticity of the earth is small, the integrals may be taken over the surface of a sphere of radius c, instead of over the ellipsoid. Therefore,

$$\mathrm{a}=h\varrho c^4 \iint \mathrm{F}(\theta, \phi) \sin\theta\,(\sin^2\theta \sin^2\phi+\cos^2\theta)\,d\theta\,d\phi,$$

$$\mathrm{M}\bar{x}=h\varrho c^3 \iint \mathrm{F}(\theta, \phi) \sin^2\theta \cos\phi\,d\theta\,d\phi,$$

$$\mathrm{M}=\tfrac{4}{3}\pi\mathfrak{D}c^3,$$

and other integrals of a like nature for b, c, D, E, F, $\bar{y}$, $\bar{z}$.

Since

$$\iint \mathrm{F}(\theta, \phi) \sin\theta \, d\theta \, d\phi = 0, \text{ therefore } \mathrm{a}+\mathrm{b}+\mathrm{c}=0.$$

If A be the moment of inertia of the body, after deformation, about an axis parallel to x, through $\bar{x}, \bar{y}, \bar{z}$,

$$A=\mathrm{A}+\mathrm{a}-\mathrm{M}(\bar{y}^2+\bar{z}^2).$$

Now a varies as $\frac{h}{\mathrm{c}}$, whilst $\mathrm{M}(\bar{y}^2+\bar{z}^2)$ varies as $\left(\frac{h}{c}\right)^2$. But the greatest elevation or depression to be treated of is about two miles, whilst the mean radius c is about 4000 miles; hence $\frac{h}{\mathrm{c}}$ cannot exceed about $\frac{1}{2000}$, and accordingly the term $\mathrm{M}(\bar{y}^2+\bar{z}^2)$ is negligible compared to a. Whence $A=\mathrm{A}+\mathrm{a}$.

In like manner, the terms introduced in the other moments and products of inertia by the shifting of the earth's centre of inertia are negligible compared to the direct changes. Thus it may be supposed that the centre of inertia remains fixed at the origin, and that the moments and products of inertia of the earth after deformation are A+a, A+b, C+c, D, E, F.

11. *General Theorem with respect to principal Axes.*

A general theorem will now be required to determine the position of the principal axes after the deformation.

Take as axes the principal axes of a body about which its moments of inertia are A, B, C. Let the body undergo a small deformation, which turns the principal axes through small angles α, β, γ about the axes of reference, and makes the new principal moments A′, B′, C′. And let the moments and products about the axes of reference become in consequence A+a, B+b, C+c, D, E, F. Then it is required to find α, β, γ in terms of these last quantities.

Let l, m, n be the direction cosines of any line through the origin, and let them remain unaltered by the deformation. Let I be the moment of inertia about this line after deformation. Let $l+\delta l$, $m+\delta m$, $n+\delta n$ be the direction cosines of the line with respect to the new principal axes. Then, by a well-known theorem,

$$\delta l=\gamma m-\beta n, \quad \delta m=\alpha n-\gamma l, \quad \delta n=\beta l-\alpha m.$$

Now

$$\mathrm{I}=(\mathrm{A}+\mathrm{a})l^2+(\mathrm{B}+\mathrm{b})m^2+(\mathrm{C}+\mathrm{c})n^2-2\mathrm{D}mn-2\mathrm{E}nl-2\mathrm{F}lm.$$

But it is also equal to

$$\mathrm{A}'(l+\delta l)^2+\mathrm{B}'(m+\delta m)^2+\mathrm{C}'(n+\delta n)^2,$$

and by substituting for δl, δm, δn, this is equal to

$$\mathrm{A}'l^2+\mathrm{B}'m^2+\mathrm{C}'n^2-2mn(\mathrm{C}'-\mathrm{B}')\alpha-2ln(\mathrm{A}'-\mathrm{C}')\beta-2lm(\mathrm{B}'-\mathrm{A}')\gamma$$

to the first order of small quantities.

Now this expression must be identical with the former for all values of l, m, n; hence putting $l=1$, $m=n=0$, $A'=A+a$, and similarly $B'=B+b$, $C'=C+c$. Wherefore also

$$\alpha=\frac{D}{C'-B'}=\frac{D}{C-B}\text{ nearly,}$$

and

$$\beta=\frac{E}{A-C},\ \gamma=\frac{F}{B-A};$$

and these are the required expressions for α, β, γ.

If, however, $B=A$, γ becomes infinite, and the solution is nugatory: but since, under this condition, all axes in the plane of xy were originally principal axes, the axes of reference may always be so chosen that F is zero absolutely; and then

$$\alpha=\frac{D}{C-A},\ \beta=-\frac{E}{C-A},\ \gamma=0.$$

Therefore the new principal axis C' is inclined to the old C at a small angle $\frac{\sqrt{D^2+E^2}}{C-A}$, and is displaced along the meridian, whose longitude, measured from the plane of xz, is $\pi+\text{arc tan}\,\frac{D}{E}$. This is the case to be dealt with in the present problem. The positions of the other principal axes will be of no interest.

12. *Application of preceding Theorem.*

To solve the problem numerically in any particular case, it will be necessary to find the integrals

$$D=h\varrho c^4\iint F(\theta,\varphi)\sin^2\theta\cos\theta\sin\varphi\,d\theta\,d\varphi,$$

$$E=h\varrho c^4\iint F(\theta,\varphi)\sin^2\theta\cos\theta\cos\varphi\,d\theta\,d\varphi.$$

If $\frac{D}{h\varrho c^4}$ and $\frac{E}{h\varrho c^4}$ be called d and e, then d and e stand for the above integrals, which depend on the distribution of surface-matter in continents and seas.

It will be convenient to use a foot as the unit for measuring h, and seconds of arc for the measurement of the inclination i of the new principal axis to the old. For this purpose the value of the coefficient $\frac{\varrho c^4}{C-A}$ may be calculated once for all. Let its value when multiplied by the appropriate factors for the use of the above units be called K*. Now

$$C-A=\tfrac{2}{3}\left(\epsilon-\frac{m}{2}\right)Ma^2=\tfrac{2}{3}(1+\tfrac{2}{3}\epsilon)\left(\epsilon-\frac{m}{2}\right)Mc^2.$$

---

* I have to thank Prof. J. C. Adams for his help with respect to the numerical data, and for having discussed several other points with me.

Then if we take $\epsilon = \cdot 0033439$, being the mean of the values given by Colonel A. R. Clarke, $m = \frac{1}{289 \cdot 66}$, and $c = 20{,}899{,}917$ feet*, $M = \frac{4}{3}\pi \mathfrak{D} c^3$, and $\frac{\mathfrak{D}}{\varrho} = 2$, we get

$$C - A = \frac{8}{3}\pi \varrho c^4 \times \cdot 0010809 \times 20{,}899{,}917$$

and

$$K = 1 \cdot 08986.$$

If, in accordance with Thomson and Tait, $\frac{\mathfrak{D}}{\varrho} = 2 \cdot 1$, $K = 1 \cdot 0380$, but I shall take K as 1·090. Then we have $i'' = Kh\sqrt{d^2 + e^2}$, where $K = 1 \cdot 090$, h being measured in feet, and i'' being the angular change in the position of the principal axis of greatest moment of inertia of the earth, due to a deformation given by $hF(\theta, \phi)$ all over the surface of the spheroid.

The angle $\frac{a + b - 2c}{C - A} \cdot t$ is clearly of the same order of magnitude as i, as it was assumed to be in Part I.

III. FORMS OF CONTINENTS AND SEAS WHICH PRODUCE THE MAXIMUM DEFLECTION OF THE POLAR AXIS.

13. *Conditions under which the Problem is treated.*

On the hypothesis of incompressibility, the effect of a deformation in deflecting the pole is exactly equivalent to the removal of a given quantity of matter from one part of the earth's surface to another. But as no continent exceeds a few thousand feet in average height, the removal is restricted by the condition that the hollows excavated, and the continents formed, shall nowhere exceed a certain depth and height. The areas of present continents and seas, and their heights and depths, give some idea of the amount of matter at disposal, as will be shown hereafter. It is interesting, therefore, to determine what is the greatest possible deflection of the pole which can be caused by the removal of given quantities of matter from one part of the earth to another, subject to the above condition as to height and depth.

14. *Problem in Maxima and Minima.*

This involves the following problem:—To remove a given quantity of matter from one part of a sphere to another, the layers excavated or piled up not being greater than k in thickness, so as to make $\sqrt{D^2 + E^2}$ a maximum, the axes being so chosen as to make $F = 0$.

If D', E' be the products of inertia referred to other axes having the same origin and axis of z as before, it may easily be shown, from the fact that $D^2 + E^2 = D'^2 + E'^2$, that $D^2 + E^2$ is greatest and equal to E'^2 for that distribution of matter which makes $D' = 0$ and E' a maximum.

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* See Thomson and Tait, Nat. Phil. pp. 648, 651.

The problem is thus reduced to the following:—Rectangular axes are drawn at the centre of a sphere of radius c; it is required to effect the above-described removal of matter, so that the product of inertia about a pair of planes through z, and inclined to xz at 45° on either side, shall be a maximum, subject to the above condition as to depth, k being small compared to c. For convenience, I refer to the plane xy as the equator, to xz as prime meridian, from which longitudes ψ are measured from x towards y, and to θ the colatitude. These must not be confused with the terrestrial equator, longitude, and latitude.

A little consideration shows that the seas and continents must be of uniform depth k, that there must be two of each, that they must all be of the same shape, must be symmetrical with respect to the equator, and that the continents must be symmetrical with respect to the prime meridian, and the seas with respect to meridians 90° and 270°.

Also the total product of inertia P, produced by this distribution, is 16 times that produced by the part of one continent lying in the positive octant of space; and the mass of matter removed is 8 times the mass of this same portion of one continent.

The problem is, therefore, to find the outline of the continent, so that P may be a maximum, subject to the condition that the mass is given.

Take the surface-density of the sphere as unity, and let the mass removed be given as an elevation of a height k over a fraction q of the whole sphere's surface; so that the mass removed from hollow to continent is $4\pi c^2 kq$. Then it may easily be shown that

$$\mathrm{P}=4kc^4\int_0^{\frac{\pi}{2}}\sin^3\theta\sin 2\psi d\theta$$

and

$$q=\frac{1}{\pi}\int_0^{\frac{\pi}{2}}2\psi\sin\theta d\theta,$$

where ψ is a function of θ to be determined. Then writing ω for 2ψ, and μ for $\cos\theta$, we have to make

$$\int_0^1\{(1-\mu^2)\sin\omega-\omega\cos^2\alpha\}d\mu \text{ a maximum,}$$

for it will be seen later that $-c^2\cos^2\alpha$ is a proper form for the constant, to be introduced according to the principles of the Calculus of Variations. This leads at once to

$$(1-\mu^2)\cos\omega=\cos^2\alpha$$

or

$$\sin^2\theta\cos 2\psi=\cos^2\alpha,$$

That is to say, the outline of the continent is the sphero-conic formed by the intersection with the sphere of the cone, whose cartesian equation is

$$y^2(1+\cos^2\alpha)+z^2\cos^2\alpha=x^2\sin^2\alpha.$$

Reverting to the expressions for P and q, altering the variable of integration, and the

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limits, so as to exclude the imaginary parts of the integrals, we have as the equation to find α

$$q=\frac{1}{\pi}\int_0^\alpha \cos\chi \operatorname{arc\,cos}\left(\frac{\cos\alpha}{\cos\chi}\right)^2 d\chi,$$

and

$$P=4kc^4\int_0^\alpha \cos\chi\sqrt{\cos^4\chi-\cos^4\alpha}\,d\chi.$$

These integrals are reducible to elliptic functions; but in order not to interrupt the argument, I give the reduction in Appendix B. If $\cos 2\gamma=\cos^2\alpha$, the result is that

$$\pi q=\sqrt{2}\,\frac{\cos 2\gamma}{\cos\gamma}\left[\Pi'(-2\sin^2\gamma)-F'\right]$$

or

$$q=1-\frac{2}{\pi}\{E'F-F'(F-E)\}$$

and

$$\frac{P}{kc^4}=\frac{8\sqrt{2}}{3}\cos\gamma\left[E'-\cos 2\gamma F'\right],$$

where the modulus of the complete functions E', F', Π' is $\tan\gamma$, and where E, F have a modulus $\frac{\cos\alpha}{\cos\gamma}$ and an amplitude $\frac{\pi}{2}-\gamma$.

It will be observed that α is the semi-length of the continent in latitude, and γ the semi-breadth in longitude.

From these expressions I have constructed the following Table:—

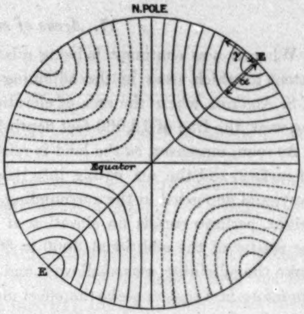
Semi-breadth of continent (γ).	Semi-length of continent (α).	Fraction of surface elevated or depressed (q).	Product of inertia $\left(\frac{P}{kc^4}\right)$.
°	° ′		
0	0	·0000	·0000
5	7 5	·0054	·0672
10	14 13	·0216	·2628
15	21 28	·0486	·5697
20	28 55	·0867	·9603
25	36 42	·1362	1·3981
30	45 0	·1979	1·8399
35	54 12	·2732	2·2371
40	65 22	......	......
45	90 0	·5000	2·6667

15. *Application of preceding problem to the case of the Earth.*

In the application to the case of the earth, what has been called, for brevity, the equator (EE in fig. 2) must be taken as a great circle, passing through a point in *terrestrial* latitude 45°.

Figure 2 gives the stereographic projection of the forms of continents and seas the firm lines showing continents, and the broken ones seas, when covering various fractions of the whole surface; α and γ are indicated on one of the continents. The other hemisphere is the same as this figure, when seen in a looking-glass. It will be observed that the limiting case is when the two continents fill up two quarters of the earth, and the two seas the other two.

Fig. 2.



It is clear that the greatest deflection of the polar axis which can be produced by the elevation of continents of height k and having a total area $4\pi c^2 q$, and the depression of similar seas, will be PK.

A numerical Table of results will be given below, formed by interpolation in the above Table.

IV. ON GEOLOGICAL CHANGES ON THE EARTH'S SURFACE.

16. *The points to be considered.*

It is now necessary to consider what kind and amount of superficial changes are brought about in the earth's shape by such geological changes as are believed to have taken place. The points to be determined are:—

i. Over what extent of the earth's surface is there evidence of consentaneous subsidence, or upheaval, during any one period.

ii. What is the extreme vertical amount of that subsidence or upheaval.

iii. How the sea affects the local excesses and deficiencies of matter on the earth's surface.

iv. How marine and aërial erosion affect the distribution of the excess or deficiency of matter.

v. The possibility of wide-spread deformations of the earth, which approximately carry the level surfaces with them.

The object of this discussion is to find what areas and amounts of elevation and subsidence on a sealess and rainless globe are equivalent, as far as moving the principal axis, to those which obtain on the earth. These areas and effects will be referred to as "effective areas and amounts of elevation or subsidence."

It is probable that during the elevation or subsidence of any large area, the change proceeds at unequal rates in different parts; probably one part falls or rises more quickly than another, and then the latter gains on the former. But it has been shown, in the dynamical part of this paper, that the axis of rotation sensibly follows the axis of figure. Hence it is immaterial by what course the earth changes its configuration, provided the changes do not proceed by large impulses, a supposition which may be certainly excluded.

The essential point is, to compare the final and initial distributions of matter, after and before a period of large geographical change.

17. *Areas of subsidence and elevation.*

When a new continent is being raised above the sea, there is no certainty as to the extent to which areas in the adjoining seas partake in the elevation; even in the case of S. America, where the area of elevation is supposed to be abruptedly limited towards the west, the line of 15,000 feet depth lies a long way from the coast.

As soon, moreover, as the land is raised above the sea, the rivers begin washing away its surface, and the sea eating into its coasts. The materials of the land are carried away, and deposited in the surrounding seas. Thus to form a continent of 1000 feet in height, perhaps entails an elevation of the surface of from 3000 to 4000 feet, and all the matter of the additional 2000 to 3000 feet is deposited in the sea. This tends to make the adjoining seas shallower, and to cause some increase to the area of the land. Therefore in a sealess globe the effect must be represented by a greater area of elevation and a less height.

The bed of a deep sea is hardly at all subject to erosion, and therefore the tendency seems to be to make the negative features of an ocean-bottom more pronounced than the positive features of mountain-ranges, at least in the parts very remote from land.

The areas, then, of existing continents may not be a due measure of the areas of effective elevation; we can only say that the latter may considerably exceed the former. The direct evidence as to the extent of the earth's surface over which there has been a general movement during any one period, is also very meagre. It appears certain that very large portions of S. America have undergone a general upward movement within a recent geological period; but there is no certainty whatever as to the limits of this area, nor as to whether the beds of the adjoining seas have partaken to any extent of this general movement. Thus the case of S. America is of scarcely any avail in determining the point in question. The presence of deep ocean up to the Chilian coast seems, however, to make it probable that areas of elevation are more or less abruptly divided from those of rest or subsidence.

There is only one area of large extent in which we possess fairly well-marked evidence of a general subsidence; and this is the area embracing the Coral islands of the Pacific Ocean. The evidence is derived from the structure of the Coral islands, and is confirmed in certain points by the geographical distribution of plants and animals. Some naturalists are of opinion that there is evidence of the existence of a previous continent; others (and amongst them my father, Mr. CHARLES DARWIN) that there existed there an archipelago of islands. In this dearth of precise information, only a rough estimate of area is possible.

My father, who has especially attended to the subject of the subsidence of the Pacific islands, has marked for me, on the map given in his work on Coral Reefs, a large area which he believes to have undergone a general subsiding motion. This area runs in a great band from the Low Archipelago to the Caroline Islands, and embraces the greater number of the islands coloured dark-blue in his map. The boundary may be defined as passing through:—

	N.							S.							
Lat.	3	5	15	22	18	10	5	5	15	25	30	18	15	10	8
Long.......	150	140	150	165	180	165	150	135	120	120	135	150	165	180	165
	E.					W.									E.

He also marked a smaller area, embracing New Caledonia, the S.E. corner of New Guinea, and the N.E. coast of Australia.

It is noteworthy that the former large area consists of sea more than 15,000 feet deep, except in patches round some of the islands, where it appears to be from 10,000 to 15,000 feet deep[*].

I marked these areas on a globe, and cut out a number of pieces of paper to fit them, and then weighed them. By this method I determined that the former area was ·055 of the whole surface of the globe, and the latter was ·01; the two together were therefore ·065.

It thus appears that we have some evidence of an area of between 5 and 7 per cent. of the globe having undergone a general motion of subsidence within a late geological period. But between this area and the coast of S. America there is a vast and deep ocean, and nothing whatever is known with respect to the movements of its bed. Hence it is quite possible that the area which has really sunk, in this quarter of the globe, is considerably larger than the one above spoken of.

On the whole, then, perhaps from ·05 to ·1 of the whole surface may at various times have partaken of a consentaneous movement, so as to convert deep sea into land, and *vice versâ*.

Besides this kind of general movement, there have certainly been many more or less local rises and falls, but this small oscillation is not fitted to produce any sensible effect on the position of the earth's axis.

18. *Amount of Elevation, and the effects of Water.*

HUMBOLDT has shown that the mean height of the present continents is a little less than 1100 feet from the sea-level[†]. But this, of course, does not give the limit to the amount of change of level. On the other hand, there are perhaps 50,000 to 80,000 feet of superposed strata at most places on the earth; but neither does this give the indication required, because the surface must have risen and fallen many times during the deposition of these strata.

But, as before pointed out, the actual upward or downward movement of land is by no means the same as its effective elevation or subsidence; for erosion causes the effective to be far slower than the actual. And the actual upward or downward movement of an ocean-bed is different from the effective; for the sea-water will flow off or in from the adjoining seas. The specific gravity of water is about one third of that of surface rock, and the local loss or gain of matter is the actual loss or gain of surface rock, less the mass of the sea-water admitted or displaced. Thus the effective downward or upward movement of a sea-bed is about $\frac{2}{3}$ of the actual; of this a more accurate estimate will be given presently.

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[*] See frontispiece-map to WALLACE's Geogr. Distrib. of Animals.

[†] Sir J. HERSCHEL seems to have doubled the height through a misconception of HUMBOLDT's meaning. The mean height of the land is in English feet: Europe, 671; N. America, 748; Asia, 1132; S. America, 1151. See a letter to 'Nature,' by Mr. J. CARRICK MOORE, April 18th, 1872.

It is fortunately not important to track the series of changes through their course; and in order to avoid the complication of doing so, the way seems to be to estimate the amount of transference of matter entailed in the conversion of a deep ocean into a continent of the present mean height.

Suppose, then, that an ocean area of 15,000 feet in depth were gradually elevated, and that the final result, notwithstanding erosion, were a continent of 1100 feet in height. Conceive a prism, the area of whose section is unity, running vertically upwards from what was initially the ocean-bed. Initially this prism contained 15,000 feet of sea-water, and finally it contains 16,100 feet of rock; so that the local gain of matter, on this unit of area of the earth's surface, is the difference between the masses of this prism, initially and finally.

Now 1·02 is the specific gravity of sea-water, and 2·75 that of surface rock; therefore the same local gain of matter, in a sealess globe, would be given by an elevation of

$$16{,}100-\tfrac{102}{275}\text{ of }15{,}000=10{,}436\text{ feet.}$$

That is to say, 10,436 feet has been the effective elevation.

I therefore adopt 10,000 feet as the effective elevation equivalent to the conversion of deep ocean into a continent; and in the examples given hereafter, where I find the deflection of the pole for various forms and sizes of continent, I shall give the results of such an assumed conversion.

19. *Wide-spread Deformations of the Earth.*

It has hitherto been assumed that the elevation of land would not affect the sea-level; but there can be no doubt but that elevations, such as those already spoken of, would do so to the extent of, say, a hundred feet. In so far, then, as this is the case, the elevation would be masked from the eyes of geologists. But if the change of form were a gradual rising over a very wide area, the level surfaces would approximately follow the form of the rocky surface. For instance, the elliptical form of the equator carries the ocean level with it; the amount of this ellipticity is such that the difference between the longest and shortest equatorial radii is 6378 feet*. So long, however, as these bulges remain equatorial they cannot affect the position of the principal axis, even should they vary in amount from time to time. But this kind of deformation, if not symmetrical

* THOMSON and TAIT, Nat. Phil. p. 648.

with respect to the equator, would alter the position of the principal axis, without leaving any trace whatever of elevation or depression for geologists to discover.

The discrepancy which is found between the ellipticity of the earth, as deduced from various arcs of meridian, is, I presume, attributable to real inequalities in the earth's form, and not entirely to errors of observation and to the elliptical form of the equatorial section. It seems, moreover, quite possible that these wide-spread inequalities may have varied from time to time.

Hence, even if the deposit of strata in the sea did not produce a continual shifting of the weights on the earth's surface, and even if geologists should ultimately come to the conclusion that there has never been any consentaneous elevation and depression of very large continents relative to the sea-level, but that the oscillations of level have always been local, it would by no means follow that the earth's axis has remained geographically fixed.

V. NUMERICAL APPLICATION TO THE CASE OF THE EARTH.

20. *Continents and Seas of Maximum Effect.*

As far as I can learn, geologists are not of opinion that there is any more reason why upheavals and subsidences should take place at one part of the earth's surface than at another. It is accordingly of interest to suppose the elevations and depressions to take place in the most favourable places for shifting the axis of figure. The area over which a consentaneous change may take place is also a matter of opinion.

The theorem in maxima and minima in Part III. makes it easy to construct a table from which that area may be selected which seems most probable to geologists. The following Table is formed by interpolation in the Table in sec. 14; the first column gives the fraction of the earth's surface over which an elevation is supposed to take place, a depression over an equal area taking place simultaneously. The second column gives the angular shift in the earth's axis of figure, due to 10,000 feet of effective elevation; as was shown in Part IV., this would convert a deep ocean into a continent. If 10,000 feet be thought too high an estimate, the last column may be reduced in any desired proportion. Lastly, fig. 2 shows the forms of these continents and seas of maximum effect.

Area of elevation *or* subsidence, as fraction of Earth's surface.	Deflection of pole for 10,000 feet effective elevation.
·001	2¼′
·005	11¼′
·01	22½′
·05	1° 46½′
·1	3° 17′
·15	4° 33⅖′
·2	5° 36⅔′
·5	8° 4½′

N.B. The area of Africa is about ·059, and of S. America about ·033 of the Earth's surface.

21. *Examples of other forms of Continent.*

I will now apply the preceding work to a few cases where the continents and seas do not satisfy the condition of giving the maximum effect.

Figures 3, 4, 5, and 6 represent the shapes of the continents as projected stereographically. The shaded parts represent areas of elevation, the dotted parts those of depression; and in the shelving continents and seas the contour lines are roughly indicated. P′ shows the new position of the pole. In every case here given d=0 and F=0.

Fig. 3. $F(\theta, \phi)=\sin 2\theta \cos 2\phi$, from $\theta=0$ to π, and $\phi=-\frac{\pi}{4}$ to $+\frac{\pi}{4}$, and zero over the rest of the globe.

$$e=2\int_0^\pi \int_{-\frac{\pi}{4}}^{\frac{\pi}{4}} \sin^3\theta \cos^2\theta \cos 2\phi \cos\phi \, d\theta \, d\phi = \tfrac{16}{45}\sqrt{2},$$

$$i''=\mathrm{K}he=\cdot 5480h.$$

If the effective elevation or depth in the middle of continent or sea be 10,000 feet, PP′=1° 31⅓′.

This is the form of continent for which $\mathfrak{P}_2$ is worked out in Appendix A.

Fig. 4. The same shape as the last, but of uniform elevation and depression of 10,000 feet.

$$e=2\int_0^{\frac{\pi}{2}} \int_{-\frac{\pi}{4}}^{\frac{\pi}{4}} \sin^2\theta \cos\theta \cos\phi \, d\theta \, d\phi = \tfrac{2}{3}\sqrt{2},$$

$$i''=\mathrm{K}he=1\cdot 028 \times h.$$

PP′=2° 51¼′ when h=10,000; an extreme supposition, as the area affected is a quarter of the whole globe.

Fig. 5. $F(\theta, \phi)=1$, from $\theta=0$ to $\frac{\pi}{2}$, and from $\phi=-\frac{\pi}{4}$ to $+\frac{\pi}{4}$, and $-\frac{1}{7}$ over the rest of the globe. This is equivalent to $F(\theta, \phi)=\frac{8}{7}$ within the above limits.

Then $$i''=\tfrac{4}{7}\times 1\cdot 028 \times h=\cdot 587 \times h,$$

and $$\mathrm{PP}'=1^\circ\ 38', \text{ when } h=10{,}000 \text{ feet.}$$

Fig. 6. $F(\theta, \phi)=\sin 2\theta \cos 2\phi$, from $\theta=0$ to $\frac{\pi}{2}$, and from $\phi=-\frac{\pi}{2}$ to $+\frac{\pi}{2}$, and zero over the rest of the globe.

$$e=2\int_0^{\frac{\pi}{2}} \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \sin^3\theta \cos^2\theta \cos 2\phi \cos\phi \, d\theta \, d\phi = \tfrac{8}{45},$$

$$i''=\mathrm{K}he=\cdot 194 \times h;\quad \mathrm{PP}'=32\tfrac{1}{3}', \text{ when } h=10{,}000 \text{ feet.}$$

On the whole, then, it appears that continents, such as those with which we have to deal, are competent to produce a geographical alteration in the position of the pole of between one and three degrees of latitude. But all these results are obtained on what I have called the hypothesis of incompressibility.

Fig. 3.

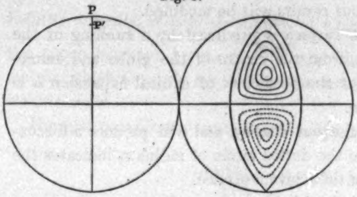


Fig. 4.

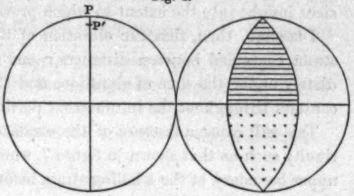


Fig. 5.

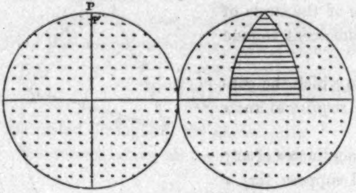
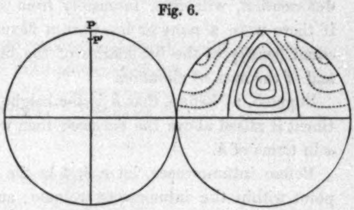


Fig. 6.



VI. HYPOTHESES OF INTERNAL CHANGES OF DENSITY ACCOMPANYING ELEVATION AND SUBSIDENCE.

22. *A general Shrinking of the Earth.*

It may be supposed that the earth is gradually shrinking, but that it shrinks quicker than the mean in some regions and slower in others. This would of course lead to depression and elevation above and below the mean surface in those regions. A deformation of this kind may be represented as a uniform compression of the earth, superposed on changes such as those considered on the hypothesis of incompressibility. If α be the coefficient of contraction of volume, it is clear that the values of D and E, as already found, must be diminished in the proportion of $1-\frac{2\alpha}{3}$ to unity, and C−A must be diminished in the like proportion. Hence the deflections of the polar axis, on this hypothesis, are exactly the same as those already found. This seems, perhaps, the most probable theory, but it is well to consider others.

The redistribution of matter caused by the erosion of continents will clearly produce the same effect as deformations on the theory of incompressibility.

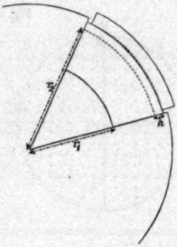
23. *Changes of Internal Density producing Elevation.*

In discussing the above hypothesis, I shall confine myself to the case of the upheaval or subsidence being of uniform height over given areas, and shall make certain other special assumptions. This will considerably facilitate the analysis, and will give sufficient insight into the extent to which previous results will be modified.

I assume, then, that the elevation of the surface is produced by a swelling of the strata contained between distances r_1 and r_2 from the centre of the globe and immediately under the area of elevation, and that the coefficient of cubical expansion α is constant throughout the intumescent portion.

This will cause a fracture of the strata of equal density, and will produce a discontinuity such as that shown in figure 7, where the dotted circle of radius r_2 indicates the upper boundary of the swelling strata before their intumescence.

Fig. 7.



But the shift of the earth's axis, caused by this kind of deformation, will differ insensibly from what would obtain if there were a more or less abrupt flexure of the strata of equal density at the boundaries of the intumescent volume and of the area of elevation.

Suppose, as before, that h is the height to which the continent is raised above the surface; then we require to know α in terms of h.

Before intumescence, let r, θ, ϕ be the coordinates of any point within the intumescent volume; and suppose that r becomes $r+u$, whilst θ and ϕ, of course, remain constant.

The equation of continuity is easily found to be

$$\frac{du}{dr}+\frac{2u}{r}=\alpha,$$

of which the integral is $ur^2=\frac{\alpha}{3}r^3+\beta$.

If β be determined, so that when $r=r_1$, $u=0$,

$$u=\frac{\alpha}{3}\frac{r^3-r_1^3}{r^2}.$$

But when $r=r_2$, $u=h$, the elevation of the surface; therefore

$$\alpha=\frac{3h}{r_2}\frac{1}{1-\left(\frac{r_1}{r_2}\right)^3},$$

the required expression for α in terms of h.

Also, before intumescence, LAPLACE's law of internal density held good, viz. $Q\frac{\sin qr}{r}$, therefore afterwards the density of the stratum distant $r+u$ from the centre is $Q(1-\alpha)\frac{\sin qr}{r}$.

Now the propositions given in Part II., as to the change in the position of the earth's axis, remain true here also; and the only difference is that the products of inertia D and E must now be expressed by different integrals.

After intumescence the earth may be conceived to consist of:—*first*, itself as it was before; *secondly*, of *negative* matter, of which the law of density is $Q\frac{\sin qr}{r}$, throughout the space bounded by $r=r_1$, $r=c$, and the cone of elevation; and, *thirdly*, of the matter which formerly lay within this space, in the configuration attained by it after intumescence.

The first part clearly contributes nothing to D and E; and the second contributes

$$-Q\iiint r^3 \sin qr \sin^2\theta \cos\theta \begin{cases}\sin\phi \\ \cos\phi\end{cases} dr\, d\theta\, d\phi,$$

integrated throughout the above space, that is from $r=r_1$ to $r=c$, and throughout the cone of elevation.

As to the third part, the mass of any element remains unchanged, whilst its distance from the centre has become $r+u$. Hence the third part contributes

$$Q\iiint r(r+u)^2 \sin qr \sin^2\theta \cos\theta \begin{cases}\sin\phi \\ \cos\phi\end{cases} dr\, d\theta\, d\phi,$$

integrated throughout the above space.

Therefore, taking all together, and treating u as small,

$$\left.\begin{matrix}\mathrm{D} \\ \mathrm{E}\end{matrix}\right\} = 2Q\iiint ur^2 \sin qr \sin^2\theta \cos\theta \begin{cases}\sin\phi \\ \cos\phi\end{cases} dr\, d\theta\, d\phi.$$

Therefore

$$\frac{\mathrm{D}}{\mathrm{d}} = \frac{\mathrm{E}}{\mathrm{e}} = 2Q\int ur^2 \sin qr\, dr,$$

where d and e have the same meanings as before, in Part II. sec. 12.

Now this last integral divides itself into two parts: first, from $r=c$ to $r=r_2$, $u=h$; and, secondly, from $r=r_2$ to $r=r_1$, $u=\frac{\alpha}{3}\cdot\frac{r^3-r_1^3}{r^2}$.

Therefore

$$\frac{\mathrm{D}}{\mathrm{d}} = \frac{\mathrm{E}}{\mathrm{e}} = 2Qh\int_{r_2}^{c} r^2 \sin qr\, dr + \tfrac{2}{3}Q\alpha\int_{r_1}^{r_2}(r^3-r_1^3)\sin qr\, dr.$$

If the value of α be substituted, and the integrations effected, it will be found that

$$\frac{\mathrm{D}}{2\mathrm{d}Qhc^3} = \frac{\mathrm{E}}{2\mathrm{e}Qhc^3} = -\frac{1}{qc\cos qc} + \frac{2}{(qc)^2}\frac{\mathrm{S}}{\sin qc} + \frac{3c}{r_2}\cdot\frac{1}{(qc)^2}\frac{1}{1-\left(\frac{r_1}{r_2}\right)^3}\frac{\mathrm{T}}{\sin qc} = \mathrm{U} \text{ suppose},$$

where S stands for the expression $\frac{r}{c}\sin qr + \cos qr$, taken between the limits c and r_2, and T for the expression $\left(\frac{r}{c}\right)^2 \sin qr + 2\frac{r}{c}\cdot\frac{1}{qc}\cos qr - \frac{2}{(qc)^2}\sin qr$, taken between the limits r_2 and r_1.

Substituting in the expression $i=\frac{\sqrt{D^2+E^2}}{C-A}$, and using the coefficient K, we get

$$i''=2KUh\sqrt{d^2+e^2}.$$

It must be noticed that this investigation is applicable as much to subsidence caused by internal compression as it is to elevation; and the word intumescence is used to cover both phenomena. In the case of subsidence h is negative.

Now on the hypothesis of incompressibility it was shown that $i''=Kh\sqrt{d^2+e^2}$. Hence, on the present hypotheses, the estimated deflection of the pole must be diminished in the proportion of 2U : 1.

Taking $qc=141°$ (which makes $\frac{\mathfrak{D}}{\mathfrak{g}}=2$, very nearly), I have calculated the values of 2U, when $\frac{r_1}{c}=\frac{399}{400}$, and $\frac{r_2}{c}=\frac{79}{80}, \frac{9}{10}, \frac{3}{4}, \frac{1}{2}, 0$. If the earth's radius be taken as 4000 miles, this gives, that the superficial strata for 10 miles in thickness do not swell, but are merely heaved up, and that the lower surface of the intumescent volume is at the various distances from the earth's surface given in the first column of the following Table. The second column gives 2U, or the factor by which previous results would have to be diminished on the present hypothesis. The third column gives the so diminished value of 1° of deflection of the pole.

Depth below surface of bottom of intumescent volume, in miles, $(c-r_2)$.	Factor of diminution of former results, (2U).	A deflection of 1° would be reduced to (2U×1°).
50	·0126	46″
400	·1011	6′ 4″
1000	·2731	16′ 23″
2000	·5171	31′ 2″
4000	·6721	40′ 20″

The last row, of course, indicates that the intumescence extends quite down to the centre of the earth.

This Table shows that if elevation is due to the swelling of strata at all near the surface, the alteration in the position of the polar axis would be reduced to quite an insignificant amount. The alleged deficiency of density under the Himalayas affords some slight evidence that it is so, at least occasionally. I believe, also, that Mr. MALLET is of opinion that the centre of disturbance of earthquake-shocks is not at a greater distance than 30 miles below the surface*. It does not, of course, follow from this evidence that there may not be elevations of both kinds going on, some being approximately superficial phenomena, and others probably due to unequal shrinking of the earth as a whole. The latter kind would be likely to produce more extensive deviations from the external form of equilibrium than the former.

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* Referred to at second hand by Mr. CARRUTHERS, Trans. New-Zeal. Inst. vol. viii. p. 363.

On the whole, then, it appears that the deflection of the polar axis cannot exceed that which was found in the case of incompressibility, and it may possibly be considerably less. The complete want of knowledge of the internal movements only allows us to state a superior limit to the change which might be produced by any one upheaval or subsidence.

VII. SUMMARY AND CONCLUSION.

24. *Summary.*

For the sake of those who do not read mathematics, I will shortly recapitulate the chief results arrived at.

The change in the obliquity of the ecliptic caused by any gradual deformation of the earth's shape of small amount is very small. Even so great a redistribution of weights on the earth's surface as is entailed by immense polar ice-caps during the Glacial Period, cannot have altered the obliquity by so much as $\frac{1}{2200}$ of a second of arc; and this is the most favourable redistribution of weights for producing this effect. Thus throughout geological history the obliquity of the ecliptic must have remained sensibly constant. And, further, when the earth undergoes any such deformation, the axis of rotation follows, and remains sensibly coincident with the principal axis of figure.

It thus only remains to consider the change in the geographical position of the poles caused by the deformation.

The principal axes at the centre of inertia of a body are three lines mutually perpendicular, and their position is entirely determined by the shape of the body. Hence if a nearly spherical body be slightly deformed, the extremities of these principal axes will move from their original positions and describe paths on the surface of the body, which may be shortly described as the paths of the principal axes. In the case of the earth, as geologically deformed, it is only of interest to consider the path of one of these axes, which is, in common parlance, the earth's axis.

If the earth be sensibly rigid, or should only readjust itself to an approximate form of equilibrium at long intervals (as maintained in Part I.), the geographical path of the axis is very nearly the same as is due merely to the geological deformation of the earth's shape; but if the earth be more or less plastic, or should readjust itself frequently to an approximate form of equilibrium, the dynamical reactions introduced are such as more or less to modify the geographical path of the axis. In the case of great plasticity these reactions would suffice to entirely alter the character of the path. It seems probable that during the consolidation of the earth there was great instability in the geographical position of the poles. Throughout the rest of the investigation suppositions of plasticity are set aside, and the hypothesis of sensible rigidity is adhered to.

Formulæ for the change in the geographical position of the pole due to any small deformation are found in Part II.

On the assumption that the internal density of the earth remains unchanged by the deformation, the forms of continent and depression which produce the greatest deflection of the poles, for the transport of a given quantity of matter from one part of the earth's surface to another, are then investigated. These forms are shown, projected stereographically, in fig. 2 (p. 293).

Part IV. gives what evidence I have been able to collect of the areas and amounts of deformation to which the earth may have been subjected in geological history; but as the discussion is not mathematical, it seems unnecessary to give an abstract thereof.

Part V. gives numerical applications of the preceding theorems to the case of the earth, on the assumption that the internal density is unaltered by the deformation. From this it appears that the poles may have been deflected from 1° to 3° in *any one geological period*; but the reader is referred back to that part for details.

If upheaval and subsidence of the surface are due to a shrinking of the earth as a whole, but to a more rapid shrinking in some regions than others, the deflection of the poles is the same as that found where there is no disturbance of the strata of equal density.

But if the upheaval and subsidence are due to local intumescence and contraction of the strata underneath the rising or falling areas, the previous numerical estimates must be largely reduced; for the extent of this reduction the reader is referred to the Table in Section 23 (p. 302).

It thus appears that the deflection of the poles first given is a superior limit to that which is possible.

25. *Conclusion.*

There remain, in conclusion, one or two miscellaneous points to be referred to.

In a letter to Sir C. LYELL read before the Geological Society*, Sir JOHN HERSCHEL has pointed out that the isothermal strata near the surface of the earth must approximately follow the solid surface. Therefore, when a thick stratum is deposited at the bottom of the ocean, the primitive bottom is gradually warmed and expands. There is thus a tendency for the upheaval of sea-beds, on which a large amount of matter has been deposited; but this kind of upheaval certainly falls within the case of superficial intumescence, and could therefore affect the geographical position of the poles but little more than would be due merely to the weight of the deposited stratum. It must be noticed, moreover, that the weight of the deposited stratum would tend to compress the primitive sea-bed, and might counteract the expansion due to rise of temperature.

If the earth were absolutely rigid the pole could never have wandered more than from 1° to 3° from its primitive position, whatever geological changes were successively to take place; because the new pole could never be brought to a greater distance from its original position, by any fresh distribution of the matter forming the continents, than the maximum for this amount of matter arranged in continents of a like height.

But it was maintained in Part I. that from time to time the earth makes a kind of rough adjustment to a figure of equilibrium. If this adjustment is, as seems probable, by an earthquake, it will take place with reference to the axis of rotation at the instant of the earthquake. Now there exists in erosion and marine deposits a cause of terrestrial deformation which is certainly independent of such adjustments; and it seems probable that the causes of geological upheaval and subsidence are so also. We have therefore clearly a state of things in which the pole may wander indefinitely from its primitive position. On this hypothesis, as in successive periods the continents have risen and fallen, the pole may have worked its way, in a devious course, some 10° or 15° away from its geographical position at consolidation, or may have made an excursion of smaller amount and have returned to near its old position. May not the Glacial Period, then, have been only apparently a period of great cold? If at that period the N. pole stood somewhere where Greenland now stands, would not the whole of Europe and a large part of N. America have been glaciated? And if the N. pole retreated to its present position, would it not leave behind it the appearance of a very cold climate having prevailed in those regions?

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* Proc. Geol. Soc. vol. ii. p. 549.

But although such a cumulative effect is possible with respect to the geographical position of the pole, none such is possible with respect to the obliquity of the ecliptic.

Now this kind of wandering of the poles would of course require extensive and numerous deformations, and it is hard to see how there can have been a shifting of the surface weights sufficient to produce it, without frequent changes in the geographical distribution of land and water. If, then, geologists are right in supposing that where the continents now stand they have always stood, would it not be almost necessary to give up any hypothesis which involved a very wide excursion of the poles?

APPENDIX A. (See p. 279.)

To calculate $\mathfrak{P}_1$ and $\mathfrak{P}_2$ in a supposed case of elevation and subsidence.

Take the case of sec. 21 (fig. 3), where the elevation is given by $ht \sin 2\theta \cos 2\phi$, from $\theta=0$ to π, and from $\phi=-\frac{\pi}{4}$ to $\frac{\pi}{4}$, and zero over the rest of the sphere. Suppose that the internal motion is entirely confined to the quarter of the sphere defined by the above limits of θ and ϕ, that radial particles are always radial, and that the motion is entirely meridional.

Let $\theta+\vartheta$ be the disturbed colatitude of the point θ, ϕ. Then the equation of continuity, which expresses that the volume of the elementary pyramid $\frac{1}{3}c^3 \sin \theta \, d\theta \, d\phi$ remains constant, when θ becomes $\theta+\vartheta$, is

$$\frac{d}{d\theta}(\vartheta \sin \theta)+\frac{3ht}{c}\sin \theta . \sin 2\theta \cos 2\phi=0,$$

the integral of which is

$$\vartheta \sin \theta+\frac{2h}{c} t \cos 2\phi \sin^3 \theta=\text{a constant};$$

and since ϑ is zero, when $\phi=\pm\frac{\pi}{4}$, for all values of t, $\vartheta=-\frac{2h}{c}t\cos 2\phi\sin^2\theta$, and

$$\frac{d\vartheta}{dt}=-\frac{2h}{c}\cos 2\phi\sin^2\theta.$$

Hence H_2, twice the area conserved on the plane of xz, is

$$\iiint \varrho r^2 \sin\theta\, dr\, d\theta\, d\phi \,.\, \frac{r^2 d\vartheta}{dt}\cos\phi,$$

taken from $r=0$ to c, $\theta=0$ to π, $\phi=-\frac{\pi}{4}$ to $+\frac{\pi}{4}$.

If the sphere be taken as homogeneous,

$$H_2=-\frac{2h}{c}\varrho\iiint r^4\sin^3\theta\cos 2\phi\cos\phi\, dr\, d\theta\, d\phi$$

$$=-\frac{16}{45}\sqrt{2}\,h\varrho c^4=-\frac{4\sqrt{2}}{15\pi}\mathrm{M}hc,$$

H_1 and H_3 are both clearly zero.

The above value of H_2 is larger than what it would be in the case of the earth, if LAPLACE's law of internal density were true. because the external layers have been taken too heavy, and the internal too light. But taking that law of density, $\mathrm{A}=\frac{1}{3}\mathrm{M}c^2$ very nearly.

Hence $\frac{H_2}{\mathrm{A}}=-\frac{4\sqrt{2}}{5\pi}\frac{h}{c}$.

If we let the time run on until the highest point of the continent has risen one foot, so that $\frac{ht}{c}=\frac{1}{20{,}900{,}000}$, then $\frac{H_2 t}{\mathrm{A}}=-\frac{4\sqrt{2}}{5\pi}\frac{1}{20{,}900{,}000}$.

But reference to sec. 21 (fig. 3) shows that $i''=\cdot 5480h$, or in the present notation,

$$\beta t=\cdot 5\times\frac{\pi}{648{,}000}\text{ nearly.}$$

Therefore

$$\frac{H_2}{\mathrm{A}\beta}=-\frac{8\times 648\sqrt{2}}{104{,}500\pi^2}=-\frac{1}{141}\text{ nearly.}$$

But generally, since the angular velocities α, β, γ of the moving axes, to which $\mathfrak{H}_1$, $\mathfrak{H}_2$, $\mathfrak{H}_3$ refer, are very small, therefore

$$\mathfrak{H}_1=H_1,\ \mathfrak{H}_2=H_2,\ \mathfrak{H}_3=H_3$$

to the first order of small quantities, within the limited period to which the investigation applies. So that in this particular case,

$$\frac{\mathfrak{H}_2}{\mathrm{A}\beta}=-\frac{1}{141}\text{ nearly, and }\mathfrak{H}_1=\mathfrak{H}_3=0.$$

And, besides, this value of $\frac{\mathfrak{H}_2}{\mathrm{A}\beta}$ is larger than it ought to be, because $\mathfrak{H}_2$ was calculated on an assumed homogeneity of the earth. This, then, justifies the conclusion in the text on p. 279.

In the elevation and subsidence given by $ht\sin 2\theta\sin 2\phi$ from $\theta=0$ to $\frac{\pi}{2}$, and from $\phi=-\frac{\pi}{2}$ to $\frac{\pi}{2}$, H_1 and H_2 are clearly zero, under a like supposition as to the nature of the internal motions accompanying upheavals.

APPENDIX B. (See p. 292.)

To reduce the integrals $\int_0^\alpha \cos\chi \operatorname{arc}\cos\frac{\cos^2\alpha}{\cos^2\chi}\,d\chi$ *and* $\int_0^\alpha \cos\chi\sqrt{\cos^4\chi-\cos^4\alpha}\,d\chi$ *to elliptic functions.*

Call the former A and the latter B.

Then integrating A by parts,

$$A=-\int_0^\alpha \sin\chi\, d\left(\operatorname{arc}\cos\frac{\cos^2\alpha}{\cos^2\chi}\right).$$

Put $x=\sin\chi$, and $\cos 2\gamma=\cos^2\alpha$, then we get

$$A=2\cos 2\gamma\int_0^{\sqrt{2}\sin\gamma}\left(\frac{1}{1-x^2}-1\right)\frac{dx}{\sqrt{x^4-2x^2+\sin^2 2\gamma}};$$

and if $x=\sqrt{2}\sin\gamma\sin\phi$, this becomes

$$\sqrt{2}\,\frac{\cos 2\gamma}{\cos\gamma}\{\Pi^1(-2\sin^2\gamma)-F^1\},\text{ where the modulus is }\tan\gamma.$$

Again, integrating B by parts,

$$B=\int_0^\alpha \sin\chi\cdot\frac{4\cos^3\chi\sin\chi\,d\chi}{2\sqrt{\cos^4\chi-\cos^4\alpha}}$$
$$=2\int_0^{\sqrt{2}\sin\gamma}\frac{(1-x^2)x^2dx}{\sqrt{x^4-2x^2+\sin^2 2\gamma}}.$$

But B is also $=\int_0^{\sqrt{2}\sin\gamma}\frac{\sin^2 2\gamma-x^2-(1-x^2)x^2}{\sqrt{x^4-2x^2+\sin^2 2\gamma}}\,dx$ from the expression before partial integration. Multiplying the latter expression by 2 and adding to the former,

$$3B=2\int_0^{\sqrt{2}\sin\gamma}\frac{\sin^2 2\gamma-x^2}{\sqrt{\sin^2 2\gamma-2x^2+x^4}}\,dx;$$

and substituting the above value for x,

$$\tfrac{3}{2}B=\frac{\sin^2 2\gamma}{\sqrt{2}\cos\gamma}F^1+\sqrt{2}\cos\gamma(E^1-F^1),$$

$$B=\frac{2\sqrt{2}}{3}\cos\gamma[E^1-\cos 2\gamma\, F^1],\text{ the modulus being }\tan\gamma.$$

B may be calculated from this form by means of the tables in LEGENDRE'S 'Fonctions Elliptiques,' tom. ii. But A is not yet in a form adapted for numerical calculation.

The parameter $-2\sin^2\gamma$ of Π^1 is negative and numerically greater than the square of the modulus; therefore Π^1 falls within LEGENDRE's second class (*op. cit.* tom. i. p. 72). Now it is shown by LEGENDRE (tom. i. p. 138) that

$$\frac{b^2\sin\theta\cos\theta}{\Delta(b,\theta)}[\Pi^1(n,c)-\mathrm{F}^1(c)]=\frac{\pi}{2}+\mathrm{F}^1(c)\mathrm{F}(b,\theta)-\mathrm{E}^1(c)\mathrm{F}(b,\theta)-\mathrm{F}^1(c)\mathrm{E}(b,\theta).$$

In this case θ will be found to be $\frac{\pi}{2}-\gamma$, $\frac{b^2\sin\theta\cos\theta}{\Delta(b,\theta)}=\frac{1}{2}\sqrt{2}\,.\,\frac{\cos 2\gamma}{\cos\gamma}$, and $b=\frac{\cos\alpha}{\cos\gamma}$;

whence $$A=\pi-2\{\mathrm{E}^1\mathrm{F}-\mathrm{F}^1(\mathrm{F}-\mathrm{E})\},$$

where the moduli of F and E are $\frac{\cos\alpha}{\cos\gamma}$, and their amplitude $\frac{\pi}{2}-\gamma$.

From this form A may be calculated numerically.

APPENDIX C. (Added April 1877.)

Sir WILLIAM THOMSON, who was one of the referees requested by the Royal Society to report on this paper, has remarked that the subject of Part I. may also be treated in another manner.

The following note contains his solution, but some slight alterations have been made in a few places.

The axis of resultant moment of momentum remains invariable in space whatever change takes place in the distribution of the earth's mass; or, in other words, the normal to the invariable plane is not altered by internal changes in the earth.

Now suppose a change to take place so slowly that the moment of momentum round any axis of the motion of any part of the earth relatively to any other part may be neglected compared to the resultant moment of momentum of the whole[*]; or else suppose the change to take place by sudden starts, such as earthquakes. Then, on either supposition (except during the critical times of the sudden changes, if any), the component angular velocities of the mass relatively to fixed axes, coinciding with the positions of its principal axes at any instant, may be written down at once from the ordinary formulæ, in terms of the direction-cosines of the normal to the invariable plane with reference to these axes, and in terms of the moments of inertia round them, which are supposed to be known.

Hence we find immediately the angular velocity and direction of the motion of that line of particles of the solid which at any instant coincides with the normal to the invariable plane at the origin. This is equal and opposite to the angular velocity with which we see the normal to the invariable plane travelling through the solid, if we, moving with the solid, look upon the solid as fixed. Let, at any instant, x, y, z be the direction-cosines of the normal to the invariable plane relatively to the principal axes;

---

[*] This is equivalent to neglecting $\mathfrak{h}_1$, $\mathfrak{h}_2$, $\mathfrak{h}_3$ of Part I.; by which Sir W. THOMSON is of opinion that nothing is practically lost.

and let A, B, C be the principal moments of inertia at that instant. Let h be the constant moment of momentum (or twice the area conserved on the invariable plane).

Consider axes fixed relatively to the solid in the positions of the principal axes at any instant, but not moving with them, if they are being shifted in virtue of changes in the distribution of portions of the solid.

The component angular velocities of the rest of the universe are, relatively to these axes, $\frac{hx}{\mathrm{A}}, \frac{hy}{\mathrm{B}}, \frac{hz}{\mathrm{C}}$; and therefore, if N be the point in which the normal to the invariable plane at the origin cuts a sphere of unit radius, the components parallel to these axes of the velocity of N relatively to them are

$$yz\left(\frac{h}{\mathrm{C}}-\frac{h}{\mathrm{B}}\right),\ zx\left(\frac{h}{\mathrm{A}}-\frac{h}{\mathrm{C}}\right),\ xy\left(\frac{h}{\mathrm{B}}-\frac{h}{\mathrm{A}}\right)\text{[*]}.$$

Now, suppose that by slow continuous erosion and deposition the positions of the principal axes change slowly and continuously relatively to the solid.

Let ϖ, ϱ, σ be the components round the axes (which, of course, are always mutually at right angles) of the angular velocity of the actual solid relatively to an ideal solid moving with the principal axes[†]. Then the component velocities relatively to this ideal solid of the point of the body coinciding at any instant with N are

$$z\varrho-y\sigma,\quad x\sigma-z\varpi,\quad y\varpi-x\varrho;$$

and the components parallel to the principal axes of the velocity of N relatively to these axes are $\frac{dx}{dt}, \frac{dy}{dt}, \frac{dz}{dt}$. Hence we have

$$\left.\begin{aligned}\frac{dx}{dt}&=yz\left(\frac{h}{\mathrm{C}}-\frac{h}{\mathrm{B}}\right)-(z\varrho-y\sigma),\\ \frac{dy}{dt}&=zx\left(\frac{h}{\mathrm{A}}-\frac{h}{\mathrm{C}}\right)-(x\sigma-z\varpi),\\ \frac{dz}{dt}&=xy\left(\frac{h}{\mathrm{B}}-\frac{h}{\mathrm{A}}\right)-(y\varpi-x\varrho).\end{aligned}\right\}\quad \ldots\ldots\ldots\quad (1)\text{[‡]}$$

These three equations give $x\frac{dx}{dt}+y\frac{dy}{dt}+z\frac{dz}{dt}=0$, and therefore they are equivalent to two independent equations to determine two of the three unknown quantities x, y, z as functions of t, the three fulfilling the condition $x^2+y^2+z^2=1$, and it being understood that ϖ, ϱ, σ are given functions of the time.

To apply these equations to the questions proposed as to the earth's axis, let the normal to the invariable plane be very nearly coincident with the axis of greatest moment of inertia C. Let O be the point where the axis C cuts the earth's surface, and let OX, OY be parallel to the axes of A and B. Then $z=1$; and if the earth's radius be taken as unity, x and y will be the coordinates relatively to OX, OY of the point P in which the normal to the invariable plane cuts the surface.

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[*] The angular velocity of the rest of the universe relatively to the earth being opposite to the angular velocity of the earth relatively to the rest of the universe, the components of the former round the axes x, y, z are taken as in the negative direction, *i. e.* from z to y, x to z, y to x.

[†] ϖ, ρ, σ are the same as $-\alpha, -\beta, -\gamma$ of Part I.

[‡] These equations are the same as those given by me in Part I. p. 277.

Putting therefore $z=1$ in the preceding equations, we find for the determination of x, y that

$$\left.\begin{aligned}\frac{dx}{dt}&=-yh\left(\frac{1}{\mathrm{B}}-\frac{1}{\mathrm{C}}\right)-\varrho+y\sigma,\\ \frac{dy}{dt}&=\quad xh\left(\frac{1}{\mathrm{A}}-\frac{1}{\mathrm{C}}\right)+\varpi-x\sigma,\end{aligned}\right\}\quad\ldots\ldots\ldots\quad(2)$$

or

$$\left.\begin{aligned}&\frac{dx}{dt}+ay=u,\\ &\frac{dy}{dt}-bx=v,\\ \text{where}\quad & a=h\left(\frac{1}{\mathrm{B}}-\frac{1}{\mathrm{C}}\right)-\sigma,\ b=h\left(\frac{1}{\mathrm{A}}-\frac{1}{\mathrm{C}}\right)-\sigma\\ \text{and}\quad & u=-\varrho,\ v=\varpi.\end{aligned}\right\}\quad\ldots\ldots\quad(3)$$

In these equations we are to regard a, b, u, v as given functions of the time.

Eliminating y, we have

$$\frac{d}{dt}\left(\frac{1}{a}\frac{dx}{dt}\right)+bx=\frac{d}{dt}\left(\frac{u}{a}\right)-v,\quad\ldots\ldots\ldots\quad(4)$$

which is a linear equation, from which x may be found by integration; and then, by the first of equations (3),

$$y=\frac{1}{a}\left(u-\frac{dx}{dt}\right).\quad\ldots\ldots\ldots\ldots\quad(5)$$

If $\mathrm{B}=\mathrm{A}$, the presence of σ in the equations would merely mean that the axes of x and y revolve with an angular velocity σ; and so we lose nothing of interest with reference to the terrestrial problem by supposing $\sigma=0$. If, then, we take A and B constant, equation (4) becomes,

$$\left.\begin{aligned}&\frac{d^2x}{dt^2}+\omega^2x=\frac{du}{dt}-av,\\ \text{where}\quad&\omega^2=ab.\end{aligned}\right\}\quad\ldots\ldots\ldots\quad(6)$$

To integrate this according to the method of variation of parameters, put

$$x=\mathrm{P}\cos\omega t+\mathrm{Q}\sin\omega t\quad\ldots\ldots\ldots\quad(7)$$

and

$$\frac{dx}{dt}=-\mathrm{P}\omega\sin\omega t+\mathrm{Q}\omega\cos\omega t.\quad\ldots\ldots\ldots\quad(8)$$

so that

$$\frac{dP}{dt}\cos\omega t+\frac{dQ}{dt}\sin\omega t=0.$$

We find then

$$\left.\begin{aligned}P&=-\frac{1}{\omega}\int\left(\frac{du}{dt}-av\right)\sin\omega t\,dt,\\ Q&=\ \ \frac{1}{\omega}\int\left(\frac{du}{dt}-av\right)\cos\omega t\,dt,\end{aligned}\right\}\quad\dots\dots\dots\quad(9)$$

For the case considered in Part I., where u and v are constant,

$$P=-\frac{av}{\omega^2}\cos\omega t+C,\quad Q=-\frac{av}{\omega^2}\sin\omega t+C',$$

and therefore by (7)

$$x=-\frac{av}{\omega^2}+C\cos\omega t+C'\sin\omega t\ .\ \dots\dots\dots\quad(10)$$

The solution expressed in equations (5), (7), (8), (9) is convenient for discontinuous as well as for continuously varying and constant values of u and v.

Consider, then, the case of $u=0$ and $v=0$, except at certain instants when u and v have infinite values, so that $\int_{T'}^{T}udt$ and $\int_{T'}^{T}vdt$ express the components of a single abrupt change in the position of the instantaneous axis; where T and T' denote any instants before and after the instant of the change, but so that the interval does not include more than one abrupt change.

Therefore, if t_0 be the instant of the change

$$\left.\begin{aligned}\int_{T'}^{T}v\sin\omega t\,dt&=\sin\omega t_0\int_{T'}^{T}vdt\\ \int_{T'}^{T}v\cos\omega t\,dt&=\cos\omega t_0\int_{T'}^{T}vdt\end{aligned}\right\}\quad\dots\dots\dots\quad(11)$$

Hence the part of x depending on v vanishes at the instant immediately after the abrupt change when $t=t_0$. Also we have by integration by parts,

$$\left.\begin{aligned}\int\frac{du}{dt}\sin\omega t\,dt&=u\sin\omega t-\omega\int u\cos\omega t\,dt,\\ \int\frac{du}{dt}\cos\omega t\,dt&=u\cos\omega t+\omega\int u\sin\omega t\,dt\end{aligned}\right\}\quad\dots\dots\quad(12)$$

And, therefore, taking the integrals between the prescribed limits, since $u=0$ both when $t=$T and when $t=$T', we have

$$\left.\begin{aligned}\int\frac{du}{dt}\sin\omega t\,dt&=-\omega\cos\omega t_0\int_{T'}^{T}udt,\\ \int\frac{du}{dt}\cos\omega t\,dt&=\ \ \ \omega\sin\omega t_0\int_{T'}^{T}udt\end{aligned}\right\}\quad\dots\dots\quad(13)$$

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Using these in (9) and (7) we find, at the instant after the abrupt change,

$$x=\int_{T'}^{T} udt \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (14)$$

and similarly

$$y=\int_{T'}^{T} vdt, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (15)$$

which of course might be deduced from (8) and (5).

IX. *The Action of Light on Selenium.*

By Professor W. G. ADAMS, *M.A., F.R.S., and* R. E. DAY, *M.A.*

Received May 18,—Read June 15, 1876.

THIS paper contains an account of a series of experiments which have been carried on by the authors during the last year and a half, and which have had for their object the investigation of the electrical behaviour of selenium, especially as regards its sensitiveness to light.

Early in the year 1873 it was discovered by Mr. WILLOUGHBY SMITH that when an electrical current was passing through a bar of crystalline selenium, its resistance was less when the bar was exposed to the action of light than it was when the bar was kept in the dark.

Through the kindness of Mr. WILLOUGHBY SMITH we have been able to follow up his remarkable discovery with, among others, the same bar of selenium with which he made his original experiments.

The objects we have had especially in view have been:—

(1) To determine whether this change in the resistance of selenium is the direct result of radiations, and if so, whether the dark-heat rays, the luminous rays, or the chemically active rays produce the greatest changes.

(2) To compare the changes of resistance in the selenium due to exposure to light from different sources and also to light which has passed through various absorbing media.

(3) To determine whether the action is instantaneous or gradual, and to measure as far as possible the intensity of the action.

(4) To examine into the character of the electrical conductivity of selenium when kept in the dark.

(5) To determine whether light could actually generate an electrical current in the selenium.

Discussion of Results previously obtained by one of us, and communicated to the Royal Society (see Proc. Roy. Soc. nos. 163 & 166).

In all the earlier experiments we used the selenium plate belonging to Mr. WILLOUGHBY SMITH. It is a plate of the substance 5·4 centims. long, 1·2 centim. broad, and ·08 centim. thick. It has been very carefully annealed, so that it is in a very fair crystalline condition, and, for selenium, conducts electricity very well. Along each end of the plate are attached platinum wires forming electrodes, and those wires are connected with binding-screws fixed on a plate of ebonite. The whole is contained in an oblong wooden box 30 centims. long, which is furnished with a draw-lid. Through the end of this box, in which there are two small holes lined with ebonite, the binding-screws are connected by means of insulated copper wires with the set of resistance-coils which are used for measuring the resistance of the selenium.

A Wheatstone's-bridge arrangement was formed, in which the four sides were the selenium, a resistance-box from 1 to 10,000 ohms, a fixed resistance of 4 ohms, and a coil of 2000 ohms—the battery electrodes being connected to the junction of the selenium and resistance-box and to the junction of the two fixed resistances, and the galvanometer electrodes being connected to the other two junctions.

In the first series of experiments, the object of which was to determine the general character of the action, the method of procedure was as follows:—

The box containing the selenium was placed on its side, and, close in front of it, was placed a black screen with a rectangular aperture, 6 × 3½ centims., opposite the selenium. On the further side of this aperture was a ledge for supporting, when necessary, coloured glasses and other absorbing media. Beyond this was placed the light whose effect was to be examined. The resistance of the selenium was then balanced as nearly as possible, without, however, permitting the current to remain on for any length of time, since its prolonged passage rapidly increases the resistance; the lid of the box was then withdrawn, and the circuit was again completed, so that the exposure and the passage of the current went on simultaneously for, in general, about 15 seconds, after which the box was closed, and then the circuit interrupted.

The resistance of this plate of selenium increases with an increase of the atmospheric temperature; but in the dark, and at ordinary temperatures, its resistance is about 2½ megohms.

When not exposed to the light the resistance of this bar of selenium begins to increase as soon as the electric circuit is completed, and continues to do so at a uniform rate, owing to the heating-effect of the current or to polarization. Although the heating-effect of the current must be small, yet the selenium is so sensitive to the slightest changes of temperature that this continuous increase of resistance may be due in part to the heating-effect of the current. On one occasion unintentionally the current from the battery was allowed to pass continuously for an hour and a half, and the resistance was found to increase continuously during the whole of that time. The rate of increase was the same as that observed when the current was allowed to pass only for a short time.

On exposure to light while the current is passing the resistance is diminished, but when the light is again eclipsed the resistance of the selenium returns in a very few minutes nearly to its previous value. This diminution of resistance is generally manifested by a strong "throw" of the galvanometer-needle towards the side of lessening resistance, followed by a more steady deflection in the same direction, which gradually attains a maximum. After this the heating and other effects of the current overbalance the action of the light, and the resistance henceforward increases steadily.

When first exposed, after being kept in the dark for some days or even hours, the selenium is much more sensitive to light than it is after repeated exposures, and hence the quantitative result obtained from the first of any series of experiments is generally not comparable with the others.

Exposure to the light of an ordinary wax taper at a distance of 20 centims. diminished the resistance of this plate of selenium by about one eighth part of the whole.

The light of an ordinary lucifer-match was found to have a powerful effect.

The diminution of resistance, when this plate of selenium was exposed to a powerful gas-light, was found in many cases to amount to from 10 to 25 per cent. of the whole resistance.

Effect of Moonlight on Selenium.

The effect of moonlight on the selenium was tried during the month of January 1875. An experiment was made first with the window open, so that the selenium was exposed to cold air as well as to moonlight, and the galvanometer-needle was deflected through 36 divisions of the scale in about two minutes, on the side indicating a diminution in the resistance of the selenium, amounting to about 18,000 ohms.

An experiment was afterwards made with the window shut, and a deflection of about 20 divisions of the scale was produced by the moonlight alone.

On another occasion when the moon was nearer full, but shining very obliquely through the window, exposure of the selenium to the moonlight produced a deflection of 150 divisions in three minutes, which was balanced by diminishing the variable resistance in the bridge by 120 ohms, showing that the resistance of the selenium was diminished by 60,000 ohms by the action of moonlight.

On another occasion, a plane mirror was placed inside a window, which was kept shut, and the moonlight was reflected upon the selenium or away from it at pleasure without approaching or in any way disturbing the selenium or its connexions. On reflecting the moonlight on to the selenium, the needle was deflected through 20 divisions of the scale very gradually; as soon as the mirror was turned away, the needle gradually fell back again. This experiment was repeated a great many times, and always with the same result. The moonlight was not bright enough to see the seconds-hand of a watch clearly. As the moonlight fell on the window at a very oblique angle, the mirror was now placed on a balcony outside the window so as to throw the reflected beam almost perpendicularly on the panes of glass (the window being kept shut during the exposures). The deflection of the needle now gradually rose to 40 divisions, and the resistance of the selenium was balanced and found to be diminished by 20,000 ohms.

On opening the window, the deflection rapidly rose on the same side to 110 divisions, and the resistance of the selenium bar was further diminished by about 57,500 ohms.

These experiments show that exposure to cold produces a change in the selenium in the same direction as exposure to moonlight or exposure to any other kind of light which has been as yet tried. They also show that the change produced in selenium by exposing it to rays of light is not due to the increase of temperature which would be caused in the selenium by those rays.

From these and a variety of other experiments which have been already described, and which point all in the same direction, we conclude that for a slight increase of temperature the resistance of the selenium is greatly increased, and for a slight lowering of temperature the resistance is greatly diminished.

The Use of a Selenium Bar as a Thermometer.

At the beginning of January 1875 there was severe frost and snow, which was followed by a sudden thaw on January 5th, when the temperature in the shade rose to 44° F. Previous to the thaw, in the room where the experiments were made, the resistance of the selenium was about 2·25 megohms; on the day of the thaw the resistance had increased to 2·7 megohms, the increase in the resistance of the selenium amounting to 450,000 ohms, or nearly one fifth of its total resistance. These results suggest that a piece of selenium kept in the dark and forming part of an electric circuit would form a very delicate thermometer, and that a very delicate differential thermometer may be formed with two similar pieces of selenium balanced against one another.

Exposure of Selenium to Radiations from the Bunsen Flame.

The effect of the ordinary non-luminous flame of a Bunsen burner was compared with that of the same flame rendered luminous by closing up the air-holes.

On exposing the selenium for several seconds to the non-luminous flame at a distance of 20 centims., only a very slight effect was observed, the deflection of the galvanometer-needle being represented by about 10 divisions of the scale; but on making the flame luminous, by closing the air-holes of the burner, the spot of light was deflected off the scale with great rapidity.

On testing for the amount of radiation from the Bunsen flame in its two states, by means of a thermopile and a galvanometer, it was found that the actual amount of heat radiated from the non-luminous flame was very nearly, but not quite, as great as that from the same flame when rendered luminous.

These experiments with the Bunsen burner were repeated in various ways, and it was always found that, while the hot non-luminous flame produced scarcely any perceptible effect, exposure to the same flame when rendered luminous produced a diminution of the resistance amounting in some cases to nearly one fourth of the whole resistance of the selenium.

In order to examine this point more fully, experiments were made with the electric light so as to determine what effect was produced by the obscure heat-rays. The selenium was placed at a distance of about 2 feet from the carbon points, and some medium which would absorb the luminous rays was placed in the path of the beam.

A plate of smoked rock-salt and a plate of alum were interposed together in the path of the beam, and, on exposing the selenium, no effect whatever was produced. This experiment was repeated several times with the same result.

The plate of smoked rock-salt was then used alone with the same result, namely, of depriving the beam of all power of affecting the selenium.

A solution of iodine in bisulphide of carbon, which appeared perfectly opaque to ordinary gaslight, was then placed in the track of the beam, and, after exposure for one minute, the diminution of resistance was represented by a deflection of 100 divisions on the galvanometer-scale. This appeared to contradict the results of the preceding experiments; but, on looking through the solution directly at the carbon points, some red light was seen.

The thickness of the solution was then increased, and it was noticed that the effect on the selenium was diminished in proportion as the solution became more and more opaque to the luminous rays.

Hence we may conclude that heat-rays do not act powerfully in reducing the electrical resistance of selenium.

The next point to be examined was as to how much of the effect produced on the electrical resistance of the selenium was due to the action of the chemical rays coming from the light to which it was exposed.

For this purpose pieces of glass of various colours and of different thicknesses were interposed between the source of light and the selenium, and it was found, as a general result, that the amount of diminution of resistance on exposure depended on the intensity of the illuminating power of the light falling upon the selenium, and not on the presence or absence of the chemically active rays.

In one series of experiments, when the source of light was a paraffin-lamp, the light was allowed, in the first experiment, to fall directly on to the selenium, and in the two subsequent ones it was sent through a ruby and an orange glass respectively in order to absorb the chemical rays—the result being that, with air alone, the diminution of resistance was 3·86 per cent., while with the ruby and the orange glasses it was 2·96 and 2·63, and with a thick piece of green glass ·7 per cent., the lessened effect being clearly due to the absorption of light by its passage through the glass.

On another occasion the light was first sent through air, and then through a solution of terchloride of phosphorus, and the consequent diminution of resistance was 700 ohms in the case of air and 650 ohms with the terchloride of phosphorus. Where water was substituted for the terchloride of phosphorus, the effect was almost exactly the same.

These experiments were repeated on several occasions, and the results obtained were in all cases similar to those we have just described.

The effects of exposure to various parts of the solar spectrum, and also of the spectrum of the electric light, were examined, and it was found that the action on the selenium was greatest in the greenish-yellow and in the red portions of the spectrum, the violet and the ultra-red rays producing very little, if any, effect.

Numerous experiments were made in order to compare the effects of exposure to light emanating from various sources. For this purpose a platinum spiral or a piece of platinum gauze, with the substance upon it whose light was to be tested, was inserted in the flame of the Bunsen burner, care being taken not to expose the selenium to the light and heat coming from the Bunsen burner itself and the lower part of the flame. Selenium, barium, thallium, sodium, salts of strontium, and sal ammoniac were thus employed, and the general character of the results obtained showed clearly that the effect on the selenium depended much more on the illuminating power of the light than on the source whence it was derived.

From the experiments already described we may fairly conclude that the luminous rays are the cause of this peculiar behaviour of selenium, and that the dark-heat rays and the chemically active rays have very little, if any, share in producing it.

The Change in the Resistance is exactly as the Square Root of the Illuminating Power.

It then became a matter of interest to know in what way the amount of change in the resistance of selenium depended upon the illuminating power of the light.

It was found that, on exposing this bar of selenium to a constant source of light at different distances, the change in its resistance, on exposure for 10 seconds (as measured by the deflection of the galvanometer-needle), was almost exactly inversely as the distance from the source of light, that is to say, directly as the square root of the illuminating power. This law was found to hold whether the source of light was 1 candle or an Argand lamp of an illuminating power equal to 16 candles.

Taking the mean of a number of experiments, all of which agreed pretty well together, the deflections at the several distances were:—

	At ¼ metre.	At ½ metre.	At 1 metre.	At 2 metres.
With Argand lamp	...	170	83	39
„ candle	...	41	18	8
„ „	82	39	18	8

Another set of experiments with the candle and the Argand lamp, which in this case had an illuminating power equal to that of 12 candles, both being at the distance of 1 metre from the selenium, gave the following results:—

With the candle the deflection was . . 19 divisions in 10 seconds.
„ „ Argand lamp the deflection was 66 „ „ „

In another series of experiments the distance of the selenium bar from the Argand burner remained unaltered, but the illuminating power was altered and at the same time measured by a Bunsen photometer:—

With 1 candle the deflection was . . . . 90 divisions.
„ 4 candles „ „ . . . . 180 „
„ 8 „ „ „ . . . . 250 „
„ 9 „ „ „ . . . . 270 „
„ 16 „ „ „ . . . . 380 „

These experiments clearly show that the change in the resistance of this bar of selenium varies directly as the square root of the illuminating power.

Diminution of Resistance with Increase of Battery-power.

At an early stage of these experiments it was found that, with the same piece of selenium and at the same temperature, the resistance diminished as the battery-power was increased. As an illustration, we may take the following observations made with Mr. W. Smith's plate of selenium:—

Number of Leclanché cells.	Resistance of the plate.
5	2·7 megohms.
35	2·2 „
5	2·7 „
30	2·3 „

And after some hours we had with

30	2·4 „

and almost immediately afterwards with

5	2·9 „

It was also found that the electrical resistance of this plate was different for currents going through it in opposite directions: that is to say, that when the selenium plate was in the dark, and the positive direction of the current was from the electrode A to the electrode B, and its resistance was balanced by the Wheatstone-bridge arrangement in the usual manner, on reversing the current so that the positive direction was B to A through the selenium, the numerical value of the balancing resistance was always found to be different from that previously obtained. If the electrical conductivity of selenium followed the ordinary law of metallic conduction, this difference would not exist; and as it seemed probable that a careful investigation of these phenomena might afford some clue to the causes of the peculiar behaviour of selenium under certain conditions, we have of late been devoting especial attention to this branch of the inquiry.

Description and Mode of Preparation of new Specimens.

In the experiments hitherto described the original plate of selenium belonging to Mr. Willoughby Smith had been exclusively employed; but as it now seemed desirable to make the inquiry more general, and not to limit it to the examination of the behaviour of one specimen only, several pieces of selenium were fitted with electrodes, and otherwise prepared for use. Each piece had a number attached to it, by which it was afterwards always distinguished, and by which it will always be referred to in the following descriptions of the experiments.

The usual method of preparing these pieces of selenium has been as follows:—A small piece, varying from a quarter-of-an-inch to an inch in length, was broken off a stick of amorphous selenium. A platinum wire was then taken, and one end bent round into a small ring, and then this ring was turned up so that its plane was at right angles to the rest of the wire. The rings of two such wires were then heated in the flame of a spirit-lamp, and pressed into the ends of the little cylinder of selenium, to act as electrodes. The whole was then annealed in the manner described below. After annealing, copper wires were soldered on to the electrodes, and the selenium was inclosed in a piece of glass tube, and the electrode passed through corks fixed in the ends of the tube by means of sealing-wax. A label with a number was then attached to one end of the tube, and the electrode nearest to this number was then always described as the "*marked*" electrode.

A few pieces were also prepared according to the method adopted by Dr. W. SIEMENS. They were formed of two spirals of platinum wire laid upon a plate of talc in such a manner that they ran parallel without touching. Upon the plate a few drops of molten selenium were let fall, and then, before solidification, another plate of talc was pressed down, and the whole heated gently, so that the wires became firmly imbedded in the mass. The protruding ends of the platinum wires served as electrodes. These were soldered to silk-covered copper wires, which were passed through a cork, and the whole was then inserted in a test-tube. A thermometer was then inserted through the cork, so that its bulb was opposite to, but not in contact with, the selenium plate.

The plates, made up according to Dr. SIEMENS's method, were annealed by placing the test-tube in a water or a paraffin bath, which was then kept for some hours at a high temperature, after which the selenium was cooled down very gradually.

The pieces made up according to the other method, and which, for the particular purposes of our inquiry, we have found to be the most convenient, were either annealed in a sand bath or in a hot-air bath. The former has been found to give the best results. The method of annealing is very simple. A large iron ball is heated to a bright red-heat in the fire and then placed in a large iron bowl filled with sand, which is heaped up all over the ball, and then left for an hour. The ball is then taken out and the pieces of selenium, wrapped up in paper, are put into the hot sand, and left there for twenty-four hours. On removing the selenium from the sand, its appearance is a sure indication whether or not the annealing has been successful; for, in the former case, the bright and glossy appearance of the amorphous selenium will have changed to a dull slate-coloured one, and when this is the case the conductivity of the specimen will, in general, be found to be very good.

In the remainder of this Paper the pieces of selenium used in each experiment will be referred to by their numbers, and, as it would not be desirable to have to describe each one of them whenever it is mentioned, we shall give here a brief account of the nature of each of the specimens which will be referred to hereafter.

Mr. WILLOUGHBY SMITH's plate, which will be referred to as W. S., has been already described.

No. 2 is a plate of selenium made up according to Dr. SIEMENS's method. It was first heated in a water bath up to 100° C., kept at that temperature for some hours, and then cooled slowly. After this its resistance at ordinary temperatures was about 120 megohms. At a subsequent period it was heated in a paraffin bath up to 205° C., kept at that temperature for 3 hours, and then cooled gradually. The next morning its resistance at a temperature of 9°·5 C. was ·402 of a megohm.

No. 3 is a plate exactly similar to No. 2, and its history is almost identically the same.

No. 4 is a plate similar in construction to Nos. 2 and 3. Its resistance before annealing was so great that we could not measure it. It was then placed in a paraffin bath, its electrodes being all the while connected with the usual arrangement for measuring its resistance. As the temperature rose its resistance gradually diminished until at 100° C. it was about 216 megohms. It was kept at this temperature for 2½ hours; but this did not seem to have permanently affected it, as, on cooling it slowly, its resistance gradually increased again, so that at 55° C. it was 11·6 megohms, and when thoroughly cool its resistance was 120 megohms. The next day it was heated up to 140° C., kept at that temperature for 3 hours, and then gradually cooled; this had a permanent effect in reducing its resistance, which at a temperature of 9° C. is now about ·193 megohm.

No. 7 is a small plate of selenium whose platinum electrodes pass through corks at the end of a piece of glass tube. A length of about three millims. at the end of a piece of platinum wire was turned round at right angles, then two such pieces were laid upon a plate of talc, a drop of molten selenium let fall upon them, and then pressed down by another plate of talc. This plate was heated for 4 hours in a hot-air bath up to 110° C., and then cooled gradually. Its resistance at ordinary temperatures is now about ·585 of a megohm.

No. 8 is a plate made up at the same time and in the same way as No. 7. Its resistance at ordinary temperatures is about ·213 of a megohm. This plate had previously been raised to 210° C., but not cooled sufficiently slowly.

No. 10 is another plate prepared at the same time and in the same way as Nos. 7 & 8. The annealing was, however, not very successful in this case, its resistance at ordinary temperatures being about 3·2 megohms.

In making up the remaining 15 specimens no talc has been employed.

No. 11 is a small piece, about 4 millims. long, of a stick of selenium made up after the manner described on page 320. Its resistance at the ordinary temperature of the air is about ·0143 of a megohm.

No. 12 is a plate made up like Nos. 7, 8, & 10. It was annealed in hot sand. Its resistance at ordinary temperatures when measured with a battery of 20 Leclanché's cells is about ·375 of a megohm.

No. 14 is a piece similar to No. 11. Its resistance at ordinary temperatures is about ·390 of a megohm.

No. 15 is a piece very similar to No. 14, but it has evidently undergone the annealing process much better than No. 14, as its resistance at ordinary temperatures is about ·025 of a megohm.

No. 20 is a small piece of selenium stick, about 2 centims. long, made up like No. 11. It was annealed in hot sand, and its resistance at 14° C. is about 905 ohms.

No. 21 is a piece of the same wire, made of the same stick, and annealed with No. 20. That it did not take the annealing so effectually is clear from the fact that its resistance at 14° C. is about 120,000 ohms.

No. 22 was made up at the same time and in the same way as Nos. 20 & 21. The annealing was more successful than in the case of No. 21, its resistance at 14° C. being about 570 ohms.

No. 23 is a small piece of a selenium stick about 4 millims. long, fitted up like No. 11. It was annealed in hot sand, and its resistance at 14° C. is about 58 ohms.

No. 24 is a piece of the same wire, made up at the same time and in the same way as No. 23. Its resistance at 14° C. is about 55 ohms.

No. 25 is a piece just like Nos. 23 & 24. Its resistance at 14° C. is about 68 ohms.

The values of the resistances of the several pieces here given must only be taken as approximate ones, and are merely intended to show the general character of the resistance of each specimen. The resistance of any particular bar of selenium is so constantly altering, owing to the action of any currents that may be sent through it, that measurements of the resistance of the same piece, at the same temperature and with the same battery-power, made on two consecutive days, will hardly ever be found to give exactly the same results. As an example of this we will give the following Table of the resistances of most of the selenium plates mentioned in this paper, and which was compiled from observations made on the 8th and 9th of May. The battery-power was in all cases 2 Leclanché's cells, and the positive pole of the battery was always connected with the marked electrode of the tube.

TABLE OF RESISTANCES.

Number.	May 8.		May 9.			
	Temperature.	Resistance in ohms.	Temperature.	Resistance in ohms.	Temperature.	Resistance in ohms.
7	14° C.	1525000	0° C.	938750	15° C.	1700000
8	„	612500	„	405000	„	670000
10	„	7600000	„	3225000	„	5800000
11	„	14900	„	16570	„	16500
14	„	460000	„	398000	„	475000
15	„	30600	„	28270	„	31300
20	„	905	„	1025	„	1140
21	„	120000	„	115000	„	123500
22	„	570	„	472	„	411
23	„	58	„	63	„	58
24	„	55	„	53	„	77
25	„	68	„	55	„	68

Definition of Direct or Positive Current as regards the Selenium.

When it was found that the resistance of the selenium varied with the direction of the current, it became very important to know what was the direction of the current in any particular case. We shall in future call those currents direct in which the positive electrode of the battery is connected with the marked electrode of the piece of selenium under examination. Such currents will be designated as positive or + currents, and those currents which go through the selenium in the opposite direction will be designated as negative or — currents.

Resistance altered by changing the Intensity or Direction of the Current.

The two phenomena of diminution of resistance with increased battery-power and change of resistance on reversing the direction of the current are closely connected and are both illustrated in the following experiments.

In order to be able to reverse the current with respect to the selenium without affecting any other portion of the circuit, the ends of the wire electrodes of the selenium were made to dip into two mercury-cups fixed on to a piece of ebonite, and these cups were connected with the binding-screws of the Wheatstone-bridge arrangement. Thus, by reversing the position of the electrodes, the direction of the current in the selenium was reversed. The positive direction of the current was always determined by means of a delicately suspended magnetic needle.

A few preliminary experiments were made in order to determine whether the change of resistance of the selenium with a change in the direction of the current depended in any way on the position of the selenium, or on the direction of the current, with respect to the magnetic meridian. No such connexion was found to exist.

Mr. WILLOUGHBY SMITH's plate was then placed in a copper air bath, and its resistance was measured with direct and reverse currents, the temperature of the inclosure being observed each time. The battery-power was kept the same, being that of 10 Leclanché's cells in series.

Direction of current.	Resistance in megohms.	Temperature.
		° C.
+	4·650	11
−	3·600	11
−	3·800	16·5
+	4·825	17
−	4·275	20
+	5·314	20
−	4·620	21
+	5·533	21

In the next series of observations the battery-power was again kept constant, being that of 20 Leclanché's cells in series, and the temperature of the inclosure was varied.

Direction of current.	Resistance in megohms.	Temperature.
		° C.
+	4·61	10·3
−	3·66	„
+	8·05	30
−	7·90	„
−	7·58	25·8
+	7·72	„
+	7·54	23·5
−	7·20	„

In the next series the temperature was kept nearly constant, while the number of cells in the Leclanché's battery was varied.

Direction of current.	Resistance in megohms.	Temperature.	Number of cells.
		° C.	
+	4·105	10·5	20
−	3·280	„	
+	4·190	10·6	15
−	3·335	„	
+	4·330	10·8	10
−	3·410	„	
−	3·670	10·9	5
+	4·450	„	

In the next series the temperature of the enclosure was kept constant at 13° C., while the battery-power was varied. It was also thought advisable to take two pairs of observations for each change of battery-power, and to note the interval of time between every two observations.

Time in minutes.	Direction of current.	Resistance in megohms.	Number of cells.
0	+	4·15	2
4	−	3·40	
2	+	4·15	
2	−	3·40	
7	−	3·34	4
2	+	4·03	
2	−	3·34	
1	+	4·03	
5	+	3·99	6
4	−	3·29	
4	+	3·98	
3	−	3·29	
8	−	3·27	8
3	+	3·94	
4	−	3·27	
4	+	3·94	
7	+	3·90	10
5	−	3·24	
5	+	3·89	
6	−	3·24	
6	−	3·22	12
4	+	3·87	
2	−	3·22	
1	+	3·87	

A set of observations made with tube No. 10, the temperature being kept constant, while the battery-power was varied, gave the following results:—

Interval in minutes.	Direction of current.	Resistance in megohms.	Number of cells.
0	+	5·65	2
1	−	6·05	
6	−	5·87	4
2	+	5·25	
1^{h} 9	+	5·07	10
7	−	5·40	
2^{h} 0	−	4·92	18
4	+	4·43	
4	+	4·41	20
2	−	4·77	

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Another and more complete set of observations made with No. 10, the temperature being constant, while the battery-power was varied, gave the following results:—

Interval in minutes.	Direction of current.	Resistance in megohms.	Number of cells.
0	−	5·50	4
2	+	5·00	
1	−	5·40	
2	+	4·90	
5	−	5·20	6
1	+	4·70	
1	−	5·15	
3	+	4·70	
6	+	4·80	8
2	−	5·13	
3	+	4·72	
2	−	5·13	
3	−	5·13	10
2	+	4·69	
1	−	5·03	
2	+	4·50	
3	+	4·62	12
1	−	4·95	
2	+	4·55	
1	−	4·91	
4	−	4·96	14
3	+	4·53	
2	−	4·93	
1	+	4·49	
4	+	4·53	16
1	−	4·90	
2	+	4·43	
3	−	4·77	
5	−	4·92	18
4	+	4·43	
1	−	4·81	
1	+	4·35	
2	+	4·41	20
2	−	4·77	
1	+	4·31	
1	−	4·74	

Another set of observations, No. 15 being the tube used, gave the following results:—

Interval between observations.	Direction of current.	Resistance in megohms.	Number of cells.
minutes.			
0	+	·03175	2
7	−	·03155	
5	+	·03191	
6	−	·03182	
8	+	·03080	3
5	−	·03035	
6	+	·03080	
8	−	·03036	
35	−	·02952	4
10	+	·03005	
9	−	·02954	
8	+	·03007	
8	+	·02932	5
7	−	·02885	
8	+	·02945	
9	−	·02900	
10	−	·02840	6
7	+	·02892	
6	−	·02852	
6	+	·02887	
9	+	·02832	7
6	−	·02809	
8	+	·02842	
7	−	·02805	
4	−	·03155	2
2	+	·03175	

A similar set of observations made with tube No. 14, but only taking two observations for each change of battery-power, gave the following results:—

Interval of the observations.	Direction of current.	Resistance in megohms.	Number of cells.
minutes.			
0	+	·5000	2
7	−	·4820	
6	−	·4770	4
7	+	·4795	
8	+	·4620	6
5	−	·4697	
5	−	·4630	8
3	+	·4560	
17	+	·4440	10
6	−	·4589	

In the second experiment with the battery of 6 cells the current was kept on for more than a minute, producing a considerable increase in the resistance; and the consequence of this was that afterwards all the resistances taken with the negative exceeded the corresponding ones taken with the positive current.

Conclusions.—From these experiments, made with different pieces of selenium, we may draw the following inferences :—

(1) That, on the whole, there is a general diminution of resistance as the battery-power is increased.

(2) That the first current sent through the selenium causes a more or less permanent "set" of the molecules, in consequence of which the passage of the current, during the remainder of the experiments, is more resisted in that direction than it is in the opposite one.

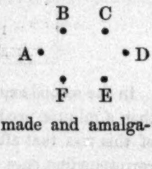
(3) That the passage of the current in any direction, at any period of the series of observations, produces a slight "set" of the molecules, which tends to facilitate the subsequent passage of a current in the opposite, but obstructs one in the same direction. Hence, when the current is sent through the selenium twice successively in the same direction, the resistance observed in the second case, even with the higher battery-power, is often equal to, or greater than, what it was in the first.

In some cases (for example, in the first of the series of observations with No. 10, see page 325) the first value of the resistance was less than one taken immediately afterwards with the current in the opposite direction. In these cases, however, it was always found, on referring to the laboratory notes, that a strong current, opposite in direction to that of the first one used, had been sent through the selenium, either a short time or sometimes as much as a day, previous to these experiments.

Polarization produced in the Selenium by the Current.

The experiments described above, and others of a similar character, seemed to indicate that the electrical conductivity of selenium is electrolytic. It was therefore important to discover whether, after the passage of an electrical current through a piece of selenium, any distinct characteristic of polarization could be detected. The existence of polarization may be considered to be established if, after the current from a voltaic battery has been passed for some time through the selenium, and then the electrodes have been disengaged from the battery and connected with a galvanometer, a current in the opposite direction to that of the battery is found to pass through the galvanometer.

In the following experiments a modification of the ordinary commutator was employed, which has answered exceedingly well and is very simple. It consists of six boxwood mercury-cups screwed into a plate of ebonite at the angles of a regular hexagon, A, B, C, D, E, F. Into the cups A and D are dipped the electrodes of the selenium bar to be tested. F and E contain the battery-electrodes, B and C the galvanometer-wires. These cups can then be connected in any way that is desirable by means of short pieces of thick copper wire. Various pieces were made and amalga-



mated at the ends, so that the change of connexions could be rapidly and easily effected.

On joining A F and D E, a simple circuit is formed through the battery and selenium; the wires A F and D E are then removed, A B and C D are joined, forming a simple circuit through the selenium and galvanometer.

The first experiment for polarization was performed with tube No. 14. The current from 20 Leclanché cells, in series, was sent through it for about a minute. The battery was then thrown out of circuit, and the selenium circuit completed through the galvanometer, when a deflection of 27 scale-divisions was obtained, indicating a current from the selenium in the opposite direction to that of the original one. On short-circuiting the selenium by means of a bit of thick copper wire across B C, the deflection of the needle at once came back to zero.

The battery-current was then sent through the selenium for about two minutes in the opposite direction to its previous one. On throwing the battery out of circuit and completing the galvanometer-circuit, a deflection of 15 scale-divisions on the other side of zero was obtained.

The next piece used was the W.S. plate. The current from 20 Leclanché cells was sent through it for two minutes, and this gave a polarization-current indicated by a galvanometric deflection of 47 scale-divisions to the right of zero. The selenium was then short-circuited, and in a few minutes all evidence of the polarization-current had disappeared. The battery-current was then put on for four minutes in the opposite direction, and this gave rise to a polarization-current represented by a deflection of 160 scale-divisions to the left of zero.

Another selenium plate, viz. that in tube No. 2, was then tried, and the same battery-current as before was sent through it for a short time. This gave rise to a distinct polarization-current, indicated by a deflection of 15 divisions to the left of zero. The same current was then sent for a considerable time through this plate in the opposite direction, and produced a polarization-current indicated by a deflection of 45 divisions to the right of zero.

That this is not due to a current arising from thermoelectric action in consequence of the junctions where the current enters and leaves the selenium being unequally heated is evident, since a current may be obtained from the selenium a considerable time after the battery-current has been interrupted, so long as the selenium electrodes have not been connected in the interval.

Here, then, we had distinct evidence of the existence of polarization when an electrical current was sent through the selenium; and it became an interesting matter to know what was the actual intensity of the currents which we were thus observing. This would be at once known if we knew the value of the current in absolute measure which, with a known electromotive force and through a known resistance, produced the unit deflection on the galvanometer-scale.

For this purpose a Daniell's cell was taken, and the porous cell was filled with a mixture of 1 part of sulphuric acid and 12 of water, the outer cell containing a saturated solution of sulphate of copper. This would give an electromotive force of ·978 volt. The resistance of this cell was very small, and that of the galvanometer with the 10th shunt about 650 ohms. The current from this cell was sent through the shunted galvanometer, and an additional interpolar resistance of 8·68 megohms, and produced a deflection of 57 scale-divisions. Hence, neglecting the resistance of the cell and of the shunted galvanometer as compared with the rest of the interpolar resistance, we have for the strength of the current in B. A. measure which, with the unshunted galvanometer, would produce the unit deflection

$$C=\frac{\cdot 978\times 10^{-1}}{57\times 8\,.\,68\times 10^{6}}=1\cdot 9767\times 10^{-10} \text{ of a B. A. unit of current.}$$

In future all currents will be expressed in scale-divisions of the unshunted galvanometer, and then, whenever it is necessary to find the value of any particular current in B. A. measure, all we have to do is to multiply the number of scale-divisions in the deflection produced by the current by the factor $1\cdot 9767\times 10^{-10}$.

A series of experiments on polarization were then undertaken with the W. S. plate.

A current of 37×10^{3} having passed through it for 10 minutes, gave a polarization-current of 45 in the opposite direction. After short-circuiting the selenium for a few minutes, no evidence of this polarization-current remained. It does not, however, follow that the original polarization was then dissipated, but that, owing to the great resistance of the circuit, the electromotive force due to the polarization, having become weakened, could no longer send any sensible current through the galvanometer. About a quarter of an hour afterwards a current of 33×10^{3} was sent for 10 minutes through the plate in the same direction as before, and the consequent polarization-current was 94 in the opposite direction.

Another series of experiments with the same plate gave the following results:—

Time.	Duration of passage.	Strength of current.	Polarization-current.
11.15 A.M.	5 minutes.	33×10^{3}	− 5
11.25 „	10 „	„	−12
11.43 „	15 „	„	−29

About an hour afterwards the current was sent through the same plate in the opposite direction, with the following results:—

Time.	Duration of passage.	Strength of current.	Polarization-current.
12.50 P.M.	5 minutes.	$24\cdot 5\times 10^{3}$	− 5·5
12.58 „	10 „	„	−13·5
1.11 „	15 „	„	−18

After the last of these experiments the electrodes of the plate were disconnected from the commutator and joined by a binding-screw, so as to allow the polarization-current gradually to subside.

On a subsequent occasion, when the current from 20 Leclanché cells had been sent through this same plate in the same direction for two successive intervals of 5 minutes, the consequent polarization-currents were —27 and —51 respectively.

A plate of selenium was next used which in outward appearance exactly resembles the last one, but which has evidently not been so well annealed, as its resistance is about 13·1 megohms. The current from the 20 Leclanché cells was sent through it for a short and also for a considerable interval of time, but no signs of a polarization-current could in either case be detected by the galvanometer.

The tubes numbered 3, 4, 8, and 12 were then tested in the same manner, with currents both of short and long duration from the battery of 20 Leclanché cells. They all showed polarization-currents, but only of feeble intensity, the deflection of the galvanometer-needle varying from 2 to 7 divisions on the scale. Though small, these currents were, however, quite distinct.

That all these currents were real polarization-currents was also evident from the fact that, on lifting any tube out of the commutator and replacing it with its electrodes reversed in the mercury-cups, the deflection was always on the opposite side of zero to what it had been previously.

The next piece used was that in tube No. 15, whose resistance is comparatively small, being about ·0247 of a megohm. The positive pole of the above battery having been connected with the marked end of the tube, the current was sent through it for five minutes, and gave rise to a polarization-current of —25. The same current was then sent through it in the same direction as before for 15 minutes, but at the end of that time the resulting polarization-current was only —11.

The next day the same tube was tried again, with the following results:—

Positive current from 20 cells for

5	minutes in at the marked end gave polarization-current of . . .	—12·5
10	„ „ „ „ „ . . .	—13·5
5	„ „ „ „ „ . . .	—10
10	„ „ unmarked end „ „ . . .	—15
10	„ „ „ „ „ . . .	—19

Tube No. 14 was then used, and the current from the above battery was sent through three times for 10 minutes at intervals of 15 minutes. The following observations were made:—

Strength of current.	Polarization-current.
250×10^3	—30
235×10^3	—25
236×10^3	—26

In the next set of experiments tube No. 11 was used. The resistance of this piece of selenium is small, being only ·0143 of a megohm. The current from 2 Leclanché cells was sent through it each time for a period of 5 minutes, and then the resulting polarization-current was measured:—

Interval between the experiments.	Polarization-current.
0	− 25
8 minutes	− 49
8 „	− 76
11 „	−112

This piece was then examined with a stronger battery-power, namely, that of 20 cells. The time during which the battery-current was kept on is indicated in the second column:—

Time when current was put on.	Duration.	Polarization-current.
1.32 P.M.	5 minutes	−550
2.5 „	3 „	−560
2.38 „	5 „	−450
2.49 „	10 „	−480

In these experiments the 10th shunt had to be used with the galvanometer, so as to keep the spot of light on the scale.

Exposure to Light alters the Strength of the Polarization-current.

All the results given by the above experiments were obtained while the selenium was kept in the dark.

In a few experiments of a similar character made with Nos. 11 and 2 an attempt was made to discover whether, on exposing the selenium to light during the passage of the *polarization-current*, any change in the intensity of that current would be produced.

In the case of No. 11, there appeared to be a slight increase in the intensity of the polarization-current during exposure, and a decrease in the intensity on shutting off the light.

An experiment with tube No. 2 showed this action rather more conclusively. A current represented by a deflection of 146×10^3 having been sent for 15 minutes through No. 2, gave a steady polarization-current of 9 divisions of the scale. On exposing the selenium to the light of the galvanometer-lamp, the deflection was at once increased to 14, and on screening off the light the deflection came back at once to 11.

The current from the 20-cell Leclanché's battery having been sent through No. 15 in the positive direction for about half a minute, gave a polarization-current indicated by a steady deflection of 25 scale-divisions to the left of zero. On exposing the selenium to the light of a candle at a distance of 6 inches, the spot of light came gradually down to zero and then moved up to the 150th division to the right of zero. On screening off the light the needle came back at once to zero, and on removing the screen it at once returned to the 150th division. The light was then held on either side of the plate, but this made no difference in the character of the action.

On screening off the light the deflection was at once reduced to its original value, and similarly the effect on exposing the selenium to light was almost instantaneous.

Here, then, seemed to be a case in which light actually produced an electromotive force within the selenium which was, in this case, opposed to and could overbalance the electromotive force due to the polarization.

Currents produced in Selenium by the action of Light.

The question of course at once presented itself as to whether it would be possible to start a current in the selenium merely by the action of light.

Accordingly, the next morning the same tube, No. 15, was placed on the commutator, and its electrodes were connected through the galvanometer. While unexposed there was no action whatever. On exposing the selenium to the light of a candle at a distance of about an inch from it, there was at once a deflection of 150 scale-divisions. On screening off the light the deflection came back at once to zero.

Hence it was clear that a current could be started in the selenium by the action of light alone.

We next made use of the original W.S. plate. The box containing it was closed, and the electrodes were placed in the cups A and D of the commutator. On completing the galvanometer-circuit there was no motion whatever of the needle. A lighted candle was then placed opposite the centre of the box, and on opening the lid the needle at once swung up and settled, so that the deflection was equal to 17 divisions of the scale. On closing the box the deflection was at once reduced to zero.

A small battery-current indicated by a reduced deflection of 35×10^2 was then sent through this plate for about half a minute, and produced a distinct polarization-current represented by a deflection of 2 divisions to the left of zero. The lid of the box was then opened, and the selenium exposed to the light of the candle. The deflection began immediately to diminish, went down to zero, and then moved up to and remained at the 10th division on the other side. On closing the box the deflection was at once reduced to zero.

The effect, on this plate, of exposure to the light of the candle at different distances was then tried, with the following results:—

I.		II.	
Distance of candle in inches.	Steady deflection.	Distance of candle in inches.	Steady deflection.
3	10	6	3·5
4	6	5	4·5
5	4	4	6
6	2	3	8

In all these experiments we found that on closing the box the deflection was almost instantly reduced to zero.

On another occasion, when the electrodes of the same piece were connected with the galvanometer, exposure to the light of burning magnesium ribbon produced a deflection of 90 divisions. The needle came back to zero as the light died away.

Tube No. 11 was then put in circuit with the galvanometer, and a gas-burner was placed at different distances from it. The selenium was then exposed, the deflection when steady of the galvanometer-needle noted, and then the light was screened off, when in all cases the deflection came back at once to zero. The results were as follows:—

Distance of burner.	Deflection.
9 inches	1
3 „	6
1½ „	14

These deflections were in the same direction whichever side or end of the selenium was exposed to the light.

On another occasion, with the same tube, exposure to the gas-flame at 8 inches distance produced a current of 4, while exposure to the light of burning magnesium ribbon at the same distance produced a current of 27.

No. 15 was then tried in the same manner with the light from a gas-burner placed at different distances.

Distance of light.	Deflection.
18 inches	11
13 „	20
7 „	64
3 „	225

The light from the burner was very variable, so that in the case of the higher deflections the needle oscillated a good deal. The above values were the mean deflections.

On another occasion, when No. 15 had its electrodes connected immediately with the galvanometer, exposure to the light of the gas-flame at a distance of 8 inches produced a deflection of 90 divisions, while exposure to the light of burning magnesium ribbon at the same distance produced a deflection of 85 with the 10th shunt in the galvanometer in the same direction as before. On again exposing to the gas-flame at the same distance, the deflection was equal to 72 divisions in the same direction. In each case the deflection came back at once to zero on screening off the light.

Action of Light on Selenium through which no Electric Current has passed.

All the pieces of selenium hitherto used had repeatedly been subjected to the action of currents passing through them, and it therefore seemed desirable to examine the effects of exposure on pieces through which no electrical current had previously been sent.

For this purpose six pieces of selenium (Nos. 20–25) inclusive (see page 322) were prepared, and carefully annealed in hot sand.

No. 20 was first connected with the galvanometer, and when screened from all light there was no deflection whatever of the needle. On exposing the selenium to the light of a candle no effect could be observed; but exposure to the light of burning magnesium ribbon produced a deflection of 3 divisions as long as the light lasted. Hence this piece is slightly sensitive to the action of light.

No. 21 was then examined in the same way, and, to cut off all the obscure heat-rays, a glass cell of water was interposed in the track of the light. Exposure to the light of a candle produced a sensible effect. The light of a gas-flame at a distance of 8 inches produced a steady deflection of 9 divisions, and the same light at 3½ inches distance a deflection of 14. In all cases the deflection came back at once to zero on screening off the light.

In another experiment with No. 21, exposure to the gas-flame at a distance of 6½ inches produced a current indicated by a deflection of 6 divisions to the right of zero. Exposure to the burning magnesium ribbon produced a strong "throw" of 40 divisions on the same side, the motion of the needle stopping at once on the light dying out.

The electrodes were then reversed in the mercury-cups, and exposure to the gas-flame produced a deflection of 5 to the left of zero, while the magnesium light produced a deflection of 56 to the left of zero. Hence it appears that, in the case of No. 21, on whichever side of the selenium the light falls, it causes a current in the same direction through the selenium.

No. 22 was then examined in a similar manner, but it seemed to be quite insensitive to the action of light.

Hence it appears that three pieces of the same length, which were made from the same rod of selenium, and which were annealed together, may, owing to some slight difference in their molecular condition, be very different as to their relative sensitiveness to the action of light.

Effect of illuminating one end only of a piece of Selenium.

In the preceding experiments the piece of selenium under examination was always exposed as a whole to the influence of the light, so that it was not possible to tell whether any one part of a piece was more sensitive than any other.

In order to examine into this point, the lime-light was used, and, by means of a lens, the light was brought to a focus on the particular portion of the selenium to be examined. A plate of glass an inch thick was interposed in the path of the beam, so as to assist in absorbing any obscure heat-rays.

Mr. W. SMITH's plate was the first examined in this way. The light was brought to bear upon the end near the marked electrode. This gave a steady deflection of 40 divisions to the right of zero, indicating a current from the marked towards the unmarked end of the selenium. On screening off the light the galvanometer-needle came back at once to zero. This was repeated several times with the same result.

The light was then brought to bear upon the centre of the plate, and then upon the end remote from the marked electrode. In neither case could any current be detected. Hence this plate appears to have one sensitive end.

No. 10 was then examined in a similar manner, but without our being able to detect any perceptible current.

No. 11, when under examination in this way, gave the following results:—

The light was brought to bear upon the marked end, and gave a current of 6 to the left of zero, *i. e.* in a direction from the selenium to the platinum at the illuminated end. Now, if platinum stands above selenium in the thermoelectric scale, the current due to heating of the junction in this case would have been from platinum to selenium, or in the opposite direction to the observed one.

When the light was brought to bear on the centre we obtained a deflection of 25 divisions to the left of zero, and then a current in the opposite direction seemed to commence, which gradually sent the deflection down to zero and away to the right. On screening off the light there was always a *throw* of the needle to the right.

The poles were then reversed in the mercury-cups of the commutator, so as to enable us to bring the light to bear upon the other side of this piece of selenium.

The light was first directed on the end remote from the mark, and gave a current of 15 to the right, *i. e.* in a direction from the platinum to the selenium.

No. 7 was then examined in the same way. When the lime-light was directed on to one end of it we obtained a steady deflection of 7 to the right of zero, and when brought to bear upon the other end a steady deflection of 4 to the left. On screening off the light, the deflection in each case came back at once to zero.

Hence in the case of No. 7 the currents produced by light falling on the two ends are in opposite directions through the selenium, and pass from platinum to selenium at the illuminated end.

No. 25 comes next. This piece had been only recently annealed, and had never previously had a current through it.

The lime-light was brought to bear upon the marked end, and produced a current of 6 from the selenium towards the platinum. The electrodes were then reversed, and the light was directed on to the end remote from the mark, and produced a current of 11 from the selenium towards the platinum at the illuminated end. In both cases we found that on screening off the light the deflection came back at once to zero.

In another experiment when the light was rather more brilliant, the currents produced by directing on to the two ends were 25 to the left and 30 to the right respectively. The direction of the current in each case was from the selenium to platinum.

Another piece, No. 23, also a new one, and through which no current had ever been sent from a battery, was next examined, with the following results:—

The lime-light falling on the marked end gave a current of 9 from the selenium to the platinum at the illuminated end.

The light falling upon the other end gave a current of 16 from the selenium to the platinum at the illuminated end.

Light falling on the central portion gave a current of 22 from the unmarked towards the marked end; but as the incidence of the light was in this case oblique, we may further mention that it was from the less towards the more illuminated portion.

No. 24, also a new piece, gave the following results:—When the lime-light was directed upon the marked end, we obtained a current of 12 from selenium to platinum at the illuminated end.

When the light was made to fall upon the central part, it gave rise to a current of 6 in a direction from the unmarked towards the marked end.

The electrodes were then reversed in the mercury-cups so as to bring the other side of the metal under the action of the lime-light.

When the light was directed on the end remote from the mark, it produced a current of 42 towards the illuminated end.

In all these experiments we found that on interposing a screen in the track of the beam the deflection of the galvanometer-needle was at once reduced to zero.

In examining No. 21 with the lime-light the following results were obtained:—

When the light was directed into the marked end it produced a current of 75 from the selenium to the platinum at the illuminated end; and when the poles were reversed in the mercury-cups, and the light was directed on to the unmarked end, it produced a steady current of 21 from the selenium to the platinum at the illuminated end. When the light was directed on the central portion there was no perceptible current.

No. 14, which has been used both for *direction* and also for *polarization* experiments, was examined in the same way with the lime-light.

When the light was brought to bear upon one face, there was a current of 30 to the left of zero, and when, by means of a reflector and a lens, the light was directed on to the rear face of the selenium, there was a current of 100 in the same direction as before.

On examining No. 15, which is a piece of selenium which shows the phenomena of polarization very well, some powerful currents were developed by the action of the lime-light. Having noticed in some previous experiments (p. 334) that this piece was very sensitive to light, the 10th shunt was put into the galvanometer-circuit.

When the light was directed on to the unmarked end, a steady deflection of 60 was obtained with the shunted galvanometer, indicating a current from selenium to platinum at the illuminated end.

The light was then directed on to the central part, and the resulting current was 9 in the same direction as before. The poles were then reversed in the mercury-cups so as to enable the light to be thrown on to the other end of this piece of selenium.

When the light was thrown on the *central part* there was a current of 50 (with the shunt) in the same direction with respect to the selenium as in the previous experiments with this piece. When the light was thrown quite on the end the resulting current was much weaker, being only 10, but in the same direction as before, through the selenium. In all these experiments we found that on shutting off the light the deflection of the galvanometer-needle came back at once to zero.

Hence the exposure of any part of this piece to light causes a current in it from the marked to the unmarked end.

To these currents, which are due to the action of light, and which seem to differ in character from thermoelectric currents, it will be convenient to give the name of *photo-electric* currents.

Is it possible to balance a weak battery-current by the action of Light?

Knowing now that light can produce an electric current in a piece of selenium when no other current is passing or has ever passed through it, and that the apparent resistance of a plate of selenium is diminished when the selenium is exposed to the action of light, it is an interesting matter to examine what will be the effect on the apparent strength of a current which was passing through a piece of selenium in the dark when a beam of light was allowed to fall upon it.

Mr. W. SMITH's plate, enclosed in its box, was therefore put in circuit with the galvanometer, and the light of burning magnesium ribbon was found, on opening the box, to produce a deflection of 90, without any shunt. The lid of the box was then closed, and the current from one Leclanché's cell was sent through the plate, and while the current was passing, the lid of the box was opened and the selenium was exposed to the light of a gas-burner at a distance of 6½ inches. The 10th shunt being in the galvanometer, the following were the observed results:—

Deflection due to current of 1 Leclanché's cell			156 to right.
ditto	ditto	and gas-flame	208 ,,

The poles of the selenium plate were then reversed in the mercury-cups without altering the actual position of the plate, and we then found that

Deflection due to current of 1 Leclanché's cell entering at the opposite end			188 to right.
ditto	ditto	and gas-light	240 ,,

The battery-current was then turned off, and the light of the gas-flame at the same distance as before produced a deflection of 12 divisions to the left (without any shunt). These experiments were important, inasmuch as they lead us to notice the following points:—

(1) That the same light at the same distance produced in both cases the same increment of current, viz. 52 with the 10th shunt to the galvanometer.

(2) That in both cases the action of the light increased the strength of the current flowing through the selenium.

These experiments with the gas-flame and the current from one Leclanché's cell were repeated several times, and gave substantially the same results as before.

With the magnesium-light in the place of the gas-flame we obtained the following results, the 10th shunt being in the galvanometer:—

Deflection due to current of 1 Leclanché's cell		180 to right.
ditto	ditto and magnesium-light	280 „
Current cut off and only magnesium-light on		11 to left.

In order to examine into this point more fully we made use of the lime-light. The current from one Leclanché's cell was sent through the selenium, from the marked end, and produced a deflection (with the 10th shunt) of 137. The lime-light was then focused on the end furthest from the marked end, and this sent the deflection up to 155. On screening off the light, the deflection was at once reduced to 150. Hence we see that though at this end (see p. 336) light produces by itself no sensible current, yet, when the battery-current is passing, it facilitates its passage.

The lime-light was then focused on the marked end, and the following results were obtained:—

Deflection due to the current of 1 Leclanché's cell		160 with 10th shunt.
ditto	ditto and lime-light	185

When the light was focused on the central part, we obtained the following results:—

Deflection due to the current of 1 Leclanché's cell		171 with 10th shunt.
ditto	ditto and lime-light	176 „ „

When the light falls on the central part, and there is no battery-current passing, there is no deflection.

Hence it appears that the action of the lime-light on any portion of this selenium plate tends to facilitate the passage of a battery-current through the plate, whatever be its direction, but that, when no battery-current is passing, the lime-light falling upon certain portions produces a small current of its own in one particular direction, viz. from the marked to the unmarked end of the selenium.

In order that the action of the photo-electric current might be comparable with the original current, it was evident that the original (or battery) current should be one of weak intensity, and also that the resistance of the circuit should be as small as possible. The tubes numbered 23, 24, and 25 were accordingly used, and a small thermopile was made use of to send a current through them.

In the first series of these experiments tube No. 25 was used.

The positive current entering the selenium at the marked end, gave a deflection on the unshunted galvanometer of 65 to the left. While this current was passing, the lime-light was focused on the marked end, and the deflection was at once reduced to 38 to the left of zero. On screening off the light the needle took a swing up to 72, and then remained steadily at 70.

The position of the battery-poles was then reversed in the mercury-cups so that the positive current should enter the selenium at the unmarked end. Before exposure, the deflection, due to the passage of the battery-current alone, was 50 to the right of zero. The lime-light was then focused on the marked end, *i. e.* the end where the current was leaving the selenium, and the resulting deflection due to the light and the current combined was 70 to the right.

In another experiment, with the electrodes in the same positions, the deflection when the selenium was not exposed, and when the battery-current alone was passing, was 35 to the right, and on focusing the light on the end where the current was leaving the selenium, the deflection was 52 to the right.

In all these cases, the action due to the light was almost instantaneous, and the deflection came back at once to its original value when the light was screened off.

In another experiment, when the positive current entered the selenium at the unmarked end, the deflection before exposure was 40 to the left of zero; and when the lime-light was focused on the same end, *i. e.* the end where the current was entering the selenium, the deflection was 20 to the left.

The battery-current was then cut off, and the same end being exposed to the action of the lime-light, a deflection of 20 to the right indicated a current of that intensity from the selenium to the platinum at the illuminated end, just as if there had been no original current.

The electrodes of No. 25 were then again reversed in the mercury-cups, so that the positive current from the battery should enter at the marked end. The deflection before exposure was 40 to the left. The light was then focused on the unmarked end, and the deflection was at once increased to 65 to the left, and back again to 42 on screening off the light. Repeating this experiment we had, before exposure, a deflection of 43 to the left, and on exposure a deflection of 70 to the left of zero.

The positive current was then sent in at the unmarked end, and produced a deflection of 30 to the right. The light being focused on the unmarked end, the deflection was reduced to 8 to the right of zero.

Tube No. 24 was next examined in the same way, with the following results:—

			Deflections.
(1) + Current entering at the marked end			50 to the left.
ditto	ditto	light on the marked end	45 „ „
(2) + Current entering at the unmarked end			40 to the right.
ditto	ditto	light on marked end	47 „ „
(3) + Current entering at the marked end			50 to the right.
ditto	ditto	light on unmarked end	60 „ „
(4) + Current in at the unmarked end			52 to the left.
ditto	ditto	light on unmarked end	44 „ „

These experiments were repeated several times with similar results, and point to the same conclusions as those made with No. 25.

Tube No. 23 was then examined in the same way, with the following results:—

		Deflections.
(1) + Current in at the marked end		55 to the left.
ditto ditto	light on marked end	47 „ „
(2) + Current in at the unmarked end		56 to the right.
ditto ditto	light on marked end	66 „ „
(3) + Current in at the marked end		62 to the right.
ditto ditto	light on unmarked end	72 „ „
(4) + Current in at the unmarked end		56 to the left.
ditto ditto	light on unmarked end	48 „ „

Hence the experiments with all three pieces point to the same conclusions, viz.:—

(1) That when light falls on the end of the selenium at which the positive current from the thermoelectric pile is entering, it *opposes* the passage of the current.

(2) That when light falls on the end of the selenium at which the positive current from the thermoelectric pile is leaving the selenium, it *assists* the passage of the current.

(3) That when no battery-current is passing, the action of light is to cause a flow of electricity from the selenium to the platinum at the illuminated junction.

We have been led to infer, from the experiments with those pieces of selenium through which no battery-current had ever been passed, that the currents thus produced in the selenium by the action of light are not thermoelectric currents due to the heating of the junctions between the platinum electrodes and the selenium, from the following considerations:—

(1) Assuming that platinum stands above selenium in the thermoelectric scale, the positive direction of the current, due to heating a junction of platinum and selenium, would be across the heated junction from the platinum to the selenium. Hence in all the above experiments the current, supposing it to have been a thermoelectric one, would always have been away from the illuminated end—that is to say, from the platinum to the selenium. In the above experiments, however, we find that, in general, the current is from the selenium to the platinum at the illuminated end, and, in those cases where this is not found to be the case, we have the current almost always in one direction with respect to the selenium, whichever end is illuminated.

(2) A thermoelectric current does not usually attain its maximum strength immediately, nor does it cease altogether immediately after the withdrawal of the source of heat. In these experiments we have invariably found that, on interposing a screen in the path of the beam of light, the current immediately ceased, and on withdrawing the screen, the current was at once renewed in its original strength. The phenomena noticed are exactly similar to those observed when a tangent-galvanometer and a galvanic cell are joined up by a key in simple circuit. On closing the circuit, the needle swings at once up to, and then oscillates about its ultimate position of rest, and stays there while the current is passing. On breaking the circuit, the needle at once returns, oscillates on either side of, and then stops at zero.

During these experiments we were frequently struck by the analogy of the two cases, and impressed with the idea that cutting off the light was, in point of fact, removing the electromotor of the current.

In these investigations we have seen:—

(1) That a slight increase of temperature of a piece of annealed selenium is accompanied by a large increase of electrical resistance.

(2) That on increasing the strength of the current through the selenium, there was a diminution in its resistance, which seems to be due to a kind of polarization which is similar in its effects to electrolytic polarization. This polarization seems to be a permanent effect, or at least to last for a long time, so long as the ends of the selenium are not short-circuited.

(3) On exposure to light while a battery-current is passing through it, the electrical resistance of a piece of annealed selenium is apparently diminished.

The experiments with the Bunsen flame, with weak sources of light, and with moonlight, seem to show that this effect is due to the illuminating-power rather than to the heating effect of the source.

(4) The apparent change in the electrical resistance is directly proportional to the square root of the illuminating-power of the light.

(5) After the battery-current is disconnected from the selenium, the strength of the current due to polarization is increased in most cases by exposure to light.

(6) Pieces of annealed selenium are, in general, sensitive to light; that is to say, that under the action of light a kind of electromotive force is developed among the molecules, which, under certain conditions, can produce an electric current through the selenium.

(7) This sensitiveness is different at different parts of the same piece of selenium.

(8) In most of the pieces which we have tested, the action of light, when there is no battery-current passing, causes a flow of electricity from the selenium to the platinum at the illuminated junction. To distinguish these currents from currents arising from any other cause, we have called them *photoelectric* currents.

APPENDIX.

Received May 17, 1877.

The place of Selenium in the Thermoelectric Scale.

In the above paper it has been assumed as a fact established by the researches of the late Dr. MATTHIESSEN (Proc. R. S. vol. ix. p. 99, 1857, and Phil. Trans. 1858) that platinum stands above selenium in the thermoelectric scale, and therefore that the direction of a thermoelectric current would be from platinum to selenium at the heated joint. It has been suggested that, considering the changes produced in selenium by the process of annealing, it would be well to try the thermoelectric properties of the several pieces used in the above experiments.

As many of these pieces are only 3 or 4 millims. in length, and as the ends of the platinum wires were melted into and buried in them, it is somewhat difficult on applying the heat to the junctions to know on which junction the heat is concentrated.

We have investigated the thermoelectric properties of sixteen of the twenty-five pieces of selenium on which we have hitherto experimented.

Effect of Exposure to Sunlight.

Mr. W. Smith's plate of selenium, enclosed in its box, was joined up as part of a Wheatstone's bridge as in the first experiments in the paper, the battery being 10 Leclanché's cells, and was placed in such a position that, on withdrawing the lid of the box, the sunlight was brought to a focus by a powerful lens on one of the junctions of selenium and platinum (the marked end of the selenium). By altering the position of the box, the sunlight could be brought to a focus on any other point of the selenium plate.

First Experiment.—The current entered the selenium plate at the marked end, the resistance being balanced by the bridge; when the sunlight was brought to a focus on the marked end, a deflection was produced to the left, showing that the resistance of the selenium was diminished, *i. e.* that the light *assists* the passage of the battery-current from the platinum to the selenium at the marked end.

Second Experiment.—The current entering the selenium as before at the marked end, and therefore passing from selenium to platinum at the unmarked end; when the sunlight was brought to a focus on the unmarked end, a deflection was produced to the left, showing that the light *assists* the passage of the battery-current from selenium to platinum at the unmarked end.

Third Experiment.—The battery-current was reversed, so that it passed from platinum to selenium at the unmarked end; when the sunlight was brought to a focus on the unmarked end, a deflection was produced to the right, showing that the light *assists* the passage of the battery-current from platinum to selenium at the unmarked end.

Fourth Experiment.—With the same arrangement as in the third experiment, when the sunlight was brought to a focus on the marked end, a deflection was produced to the right, showing that the light *assists* the passage of the battery-current from selenium to platinum at the marked end.

In all four experiments, the light *assists* the passage of the battery-current whether it is passing from platinum to selenium or from selenium to platinum at the illuminated junction.

Next, to determine the effect produced by sunlight when there is no battery-current:—

Fifth Experiment.—The plate of selenium was joined up in simple circuit with a galvanometer only. When the sunlight was brought to a focus on the marked end, there was a deflection of 30 divisions to the left, showing that the light gave rise to a current of its own from platinum to selenium at the marked end. This current ceased when the light was removed.

Sixth Experiment.—When the sunlight was brought to a focus on the unmarked end, there was a deflection of 7 divisions to the right, showing that the light gave rise to a current from platinum to selenium at the unmarked end. Hence in this plate of selenium sunlight produces a current from platinum to selenium at the illuminated end, but the marked is much more sensitive than the unmarked end.

This agrees with previous experiments described in the paper, with the exception that the lime-light was not sufficiently powerful to give rise to a current when it was brought to a focus on the unmarked end.

It will be seen from the first four experiments just described, the results of which entirely agree with our previous investigations, that the action of sunlight on the selenium produces a very singular effect when a current from a battery is passing through it; in all cases it seems to assist the battery-current in whatever direction it is passing, so that the resistance of the selenium appears to be diminished; and yet, when there is no battery-current, sunlight causes a current from platinum to selenium at the junction on which it falls.

To Mr. WILLOUGHBY SMITH's plate, which is 1½ inch in length, heat was applied by holding each of the junctions in turn between the finger and thumb, the connecting wires being attached to a galvanometer. When the marked end was heated, a current was produced from the platinum to the selenium at the heated junction. When the unmarked end was heated, less effect was produced, but the current was from the platinum to selenium at the heated junction.

The results of the experiments just described agree with the results of the investigations of Dr. MATTHIESSEN on the thermoelectric properties of selenium, which led him to place platinum above selenium in the thermoelectric scale.

For the smaller pieces the following method of heating the junctions was adopted. A glass tube about 1 centim. in diameter was drawn out to a very fine point, and a source of heat, usually one or more Bunsen burners, placed underneath it, the fine point being placed opposite to and directed towards the junction to be heated; a blast of air was then sent into the large end of the tube, which became heated by the tube, and issued as a hot blast at the fine point where it fell upon the junction of selenium and platinum or upon any required point. On applying this method of heating to Mr. WILLOUGHBY SMITH's plate at the marked end a current was produced from platinum to selenium, and on heating the unmarked end a current was produced from platinum to selenium at the unmarked end. Hence platinum stands above this piece of selenium in the thermoelectric scale.

Other specimens of selenium were then tried in the same way, and, where it was possible, heat was also applied by holding the junction of platinum and selenium between the finger and thumb.

In nearly all those pieces of selenium with which the investigations described in the early part of this paper were carried on, a current was found to be produced at the heated junction from platinum to selenium, showing that the changes through which they had passed in the process of annealing had not raised these pieces of selenium above platinum in the thermoelectric scale. Some of these pieces which gave no current or very little current when their junctions were exposed to the lime-light, gave stronger thermoelectric currents when their junctions were held between the finger and thumb. The pieces which stand below platinum in the thermoelectric scale are Mr. W. SMITH'S plate, and Nos. 4, 7, 8, 10, & 14.

On examining No. 11, the current produced on warming the marked end by the finger gave a deflection of 15 divisions in the direction from selenium to platinum at the heated end. On warming the unmarked end by the finger there was a deflection of 110 divisions, indicating a current from selenium to platinum at the heated end.

On heating the marked end by the hot blast, there was a deflection of 55 divisions, indicating a current from selenium to platinum at the heated end; and on heating the unmarked end, there was a deflection of 115 divisions, indicating a current from selenium to platinum at the heated end.

The unmarked end of this piece is much more sensitive than the marked end.

On referring to the experiments with lime-light, it appears that the unmarked end was more sensitive to lime-light than the marked end, but that when the light was brought to a focus on the unmarked end, there was a current produced in a direction from platinum to selenium at the illuminated junction. Thus there seems in this case to be a difference in character between the action of the heat of the hand, or of the blast, and the action of the lime-light.

This is the first instance in which we have found that the process of annealing has placed selenium above platinum in the thermoelectric scale.

On referring to the paper, we see that after sending a current from a battery through this piece of selenium very strong polarization-currents were obtained from it, and that on exposure to light there was a slight increase in the intensity of the polarization-current.

No. 15 was the first piece from which a current was obtained by the action of light alone. It shows polarization-currents very well, and in it we first found that exposure to the light of a candle produced a current which was opposed to and greatly overbalanced the polarization-current. This piece is very singular in its action when its junctions are heated.

It will be convenient to call a current giving a deflection of 50 divisions of the scale a current of 50, and to denote platinum and selenium by P and S respectively.

On applying heat to one of the junctions by touching it with the hand, a current was produced from platinum to selenium at the heated junction. On warming the other junction in the same way, the current was reversed, so that, in both cases, at the heated junction the current passed from the platinum to selenium.

On repeating the experiment, with the marked end heated by the hand, there was at first a current giving a deflection of 120, or a current of 120, from P to S at the heated junction; this diminished to 50. When the hand was removed there was a deflection of 50 on the other side of zero, and the needle rested for some time at 10, showing a

4

current from S to P at the end which had been heated. With the unmarked end heated by the hand there was at first a current of 90 from S to P, which was replaced by a current of 90 from P to S, while the finger was kept on the junction. On removing the finger the current diminished to 30 from P to S, and then very slowly back to zero.

Experiments were also made on this piece with the hot-air blast.

On heating the marked end there was a current of 100 from P to S; on continuing the blast the current diminished to 20 in the same direction, and then increased again.

On heating the unmarked end there was at first a current of 150 from S to P at the heated end, which diminished to zero, and then increased again on the same side to 50 while the blast lasted. On stopping the blast there was a current of 60 in the opposite direction, which rapidly fell to zero.

These experiments were repeated.

On heating the unmarked end there was, first, a current of 75 from S to P at the heated junction, and then a current of 60 from P to S while the hot blast lasted. The current rapidly fell to zero when the blast was removed. Figure 1 is intended to represent these changes, currents being measured by deflections to right and left, and time downwards.

Fig. 1.

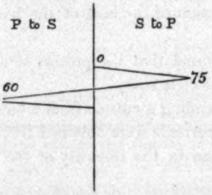
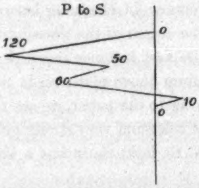
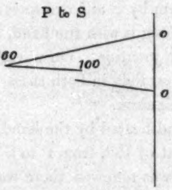


Fig. 2.



On heating the marked end we have the changes represented in fig. 2, the break denoting the stopping of the blast—a current of 120, which fell to 50 and then rose to 60 while the blast lasted. When the blast was removed there was a current of 10 in the opposite direction.

Fig. 3.



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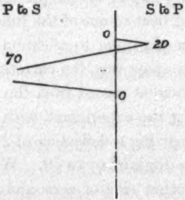


Fig. 3 represents another experiment, with the marked end heated; and fig. 4 an experiment with the unmarked end heated.

It appears from most of these experiments that on heating the marked end there is a current from P to S at that junction, and on heating the unmarked end there is a current from S to P at the heated junction. Hence, on heating the whole piece, it would appear that a continuous current is produced. The whole piece may be used as a thermoelectric pile, and would form a very delicate thermometer, since the exposure of either end of this piece to heat causes a current in it from the marked to the unmarked end. This agrees with the results of exposure to the lime-light, as recorded in the paper.

On referring to the experiments with lime-light (p. 337), we see that, when the light was brought to a focus on any part of this piece, there was a current in it from the marked to the unmarked end.

In one or two other pieces of selenium we have found that heat applied at one end produced a current from platinum to selenium, and heat applied at the other end produced a current from selenium to platinum; so that, on the whole being heated, there was a current from the first to the second end through the selenium.

The other pieces of selenium whose thermoelectric properties have been tested are Nos. 18, 20, 21, 22, 23, 24, and 25. At each junction of each of these pieces it was found that the application of heat gave rise to a current from selenium to platinum, so that the process of annealing had in these cases raised selenium from the bottom and placed it above platinum in the thermoelectric scale.

In most instances the currents due to heating the two ends did not differ much from one another. No. 21 is exceptional.

On warming with the hand the unmarked end there was a current of 10 only, whereas on applying the same source of heat to the marked end there was a current of 150. With the hot blast the values were 15 at the unmarked end and 65 at the marked end.

It seems to be pretty well established by these experiments that the more complete the process of annealing, the higher selenium is raised in the thermoelectric scale; and on referring to the Table of Resistances (p. 323), we see that the more the resistance of a piece of selenium is diminished the higher is it raised in the thermoelectric scale.

Thus all the earlier pieces, except Nos. 11 and 15, were of high resistance.

Nos. 11 and 15 are of smaller resistance, but appear to have been unequally annealed at the two ends, so that one end is above, or near, platinum, whilst the other is below platinum in the scale.

All the later specimens appear to have been well annealed at both ends, and are of very small resistance, except No. 21, which was of high resistance, and which appears to have been only partially annealed at one end at the time of the experiment.

It would seem from these experiments that the process of annealing raises selenium in the thermoelectric scale at the same time that it diminishes its electrical resistance.

Selenium changed in Character by the action of time.

Since these investigations were made in December last, the pieces of selenium have been placed in a box and laid aside, and no experiment was made with them until May 1877. On determining their resistances afresh, and repeating some of the experiments, we find that some of the pieces have changed very greatly, both in resistance and even in their position in the thermoelectric scale, through the action of time.

Those pieces which were at first not so completely annealed, and which were still of high resistance, appear to have become more completely annealed, and their resistance is very greatly diminished. This change seems to be accompanied by a rise of selenium in the thermoelectric scale.

In the following Table the resistances of several pieces, as determined in May 1876 and May 1877, are compared:—

Table of Resistances.

Number.	Resistance in ohms.	
	May 1876.	May 1877.
No. 7.	1525000	3950
„ 8.	612500	5000
„ 10.	7600000	745
„ 11.	14900	19000
„ 14.	460000	207000
„ 21.	120000	1123
„ 22.	570	272
„ 24.	55	60
„ 25.	68	28·5

Immediately after sending currents through these pieces, even from a single Leclanché cell, their resistances are usually increased and change very rapidly.

Those pieces (Nos. 7, 8, 10, and 14) which in December last were below platinum in the thermoelectric scale have risen higher in the scale.

No. 7.—On heating the marked end by contact with the finger there is a current of 240 from platinum to selenium at the marked or heated junction; but on heating the unmarked end there is a current of 200 from selenium to platinum at the unmarked or heated junction.

On heating the whole there was a current of 190 from the marked to the unmarked end.

No. 8.—On heating the unmarked end there is a strong current from platinum to selenium at the heated junction (the needle was deflected beyond the scale); on heating the marked end there was a current of 70 from selenium to platinum at the heated end. Repeating the two experiments with a shunt to the galvanometer, the deflections were 70 and 9 respectively.

No. 14.—This ranks with Nos. 7 and 8. On heating the marked end there is a current of 50 from platinum to selenium at heated end; and on heating unmarked end there is a current of 70 from selenium to platinum at the heated end. On heating the whole of the piece there was a current of 50 from the marked to the unmarked end. This piece has a resistance of 207,000 ohms for a direct current, and 195,000 ohms for a reverse current.

No. 10.—The resistance of this piece is now only one ten-thousandth part of its value a year ago, and its place in the thermoelectric scale is completely altered. Whether the heat be applied at the marked or at the unmarked end, there is a current of 90 from the selenium to platinum at the heated end. On heating the whole piece there is no effect.

Experiments have been made with Nos. 21, 22, and 24 to determine whether their resistance is diminished on exposure to light while a battery-current is being sent through them.

In all these pieces it was found that on exposing either end, or on exposing the whole to light the resistance was diminished; but in the case of No. 24 the resistance was diminished from 60 to 50 ohms, and then increased to 54 before the light was shut off; and on shutting off the light the resistance increased to 67 ohms.

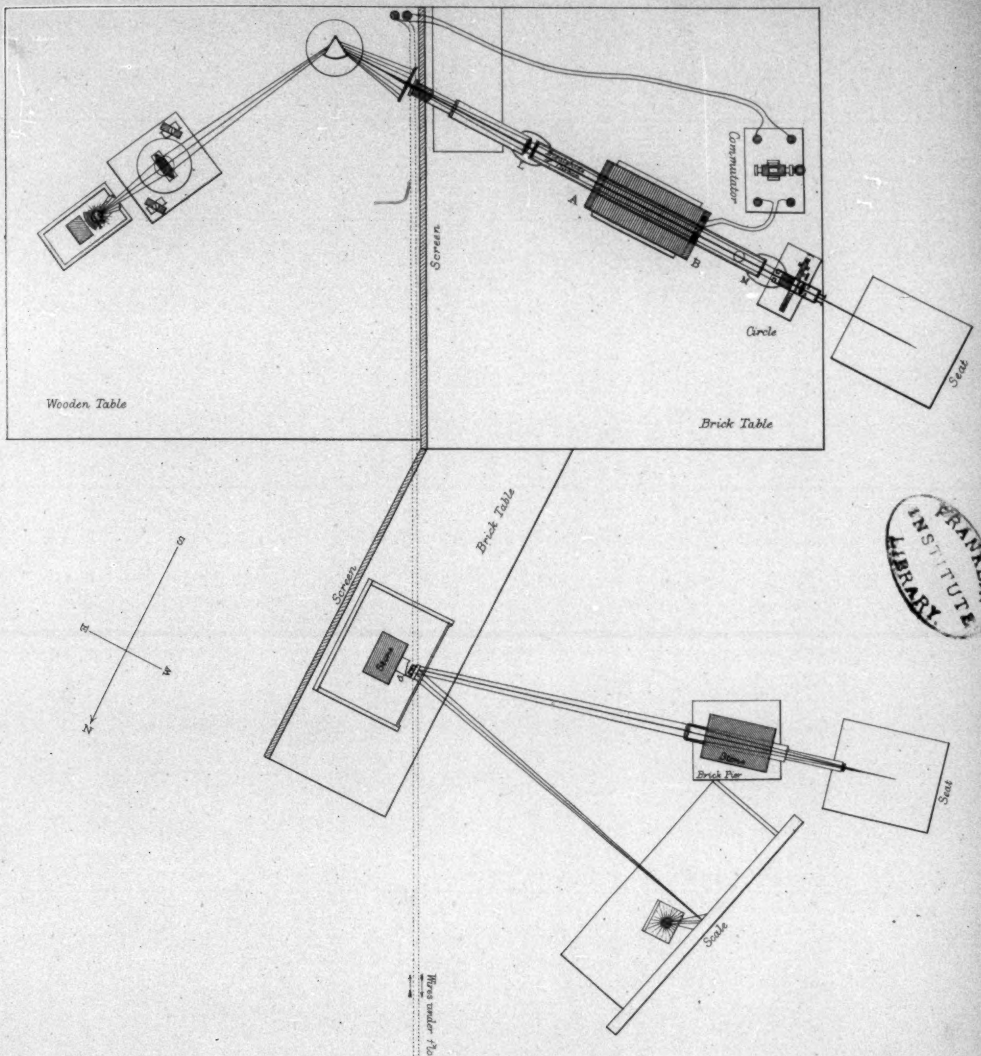
No. 8.—After the last experiments with this piece, which have been described above, its resistance was found to be 5290 ohms.

The two balancing-resistances in the Wheatstone's bridge were equal, and each 100 ohms. On connecting the marked end to the positive pole of the battery, and exposing the marked end to the lime-light, the resistance was increased, the needle being deflected beyond the scale. On exposing the unmarked end the resistance was also increased, the needle being again deflected off the scale in the same direction. On exposing the whole piece to the lime-light, the resistance was also increased, and was found to be 6290 ohms on balancing with the bridge. On shutting off the light, the resistance at once began to diminish; it had diminished to 5790 ohms in about 1 minute, and remained for some time at about 5650 ohms.

On allowing the piece time to recover its former state, and then reversing the battery-current, the resistance was again increased by about 1000 ohms on exposing the whole piece to the action of the lime-light, and the resistance was also increased on exposing either end. On shutting off the light the resistance again rapidly diminished.

This piece in its present state is different in character from every other piece with which we have experimented, in that its electrical resistance is increased when it is exposed to the action of light during the passage of an electric current through it.

Gordon. Phil. Trans. 1877. Plate 1



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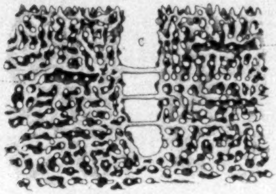

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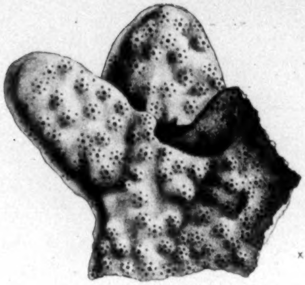

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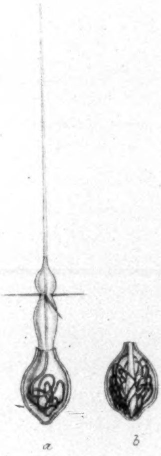

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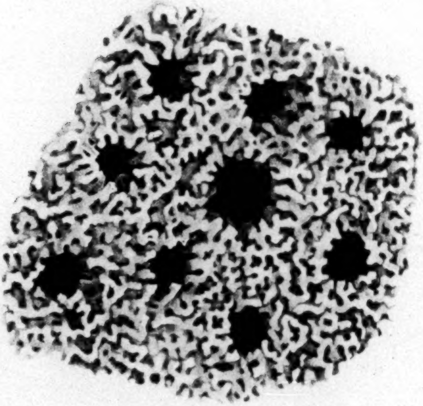

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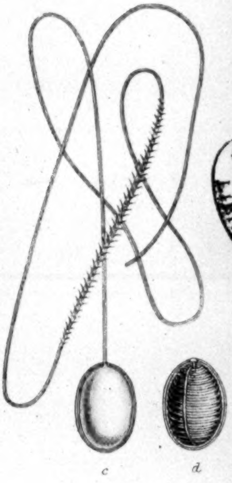

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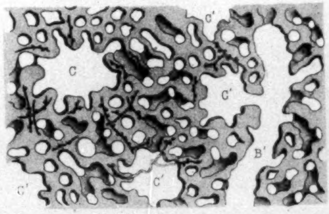

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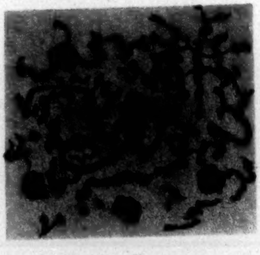

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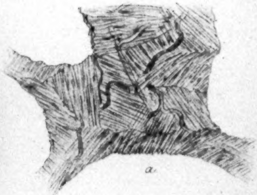

Fig. 8.

J. J. Wild & H. N. Moseley, del.

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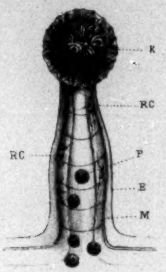

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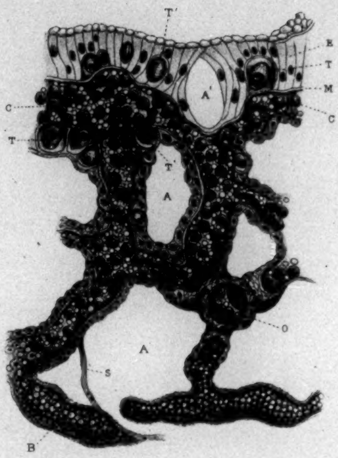

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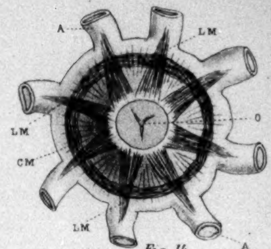

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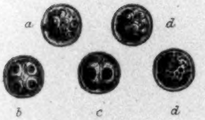

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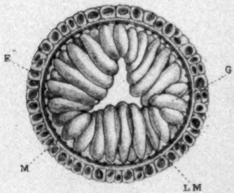

Fig. 15.

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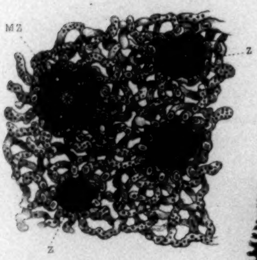

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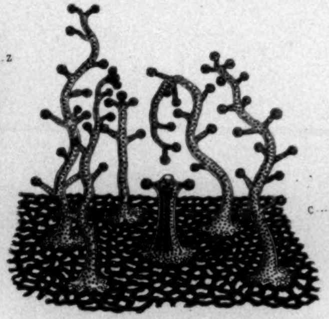

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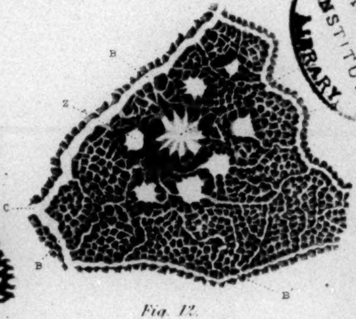

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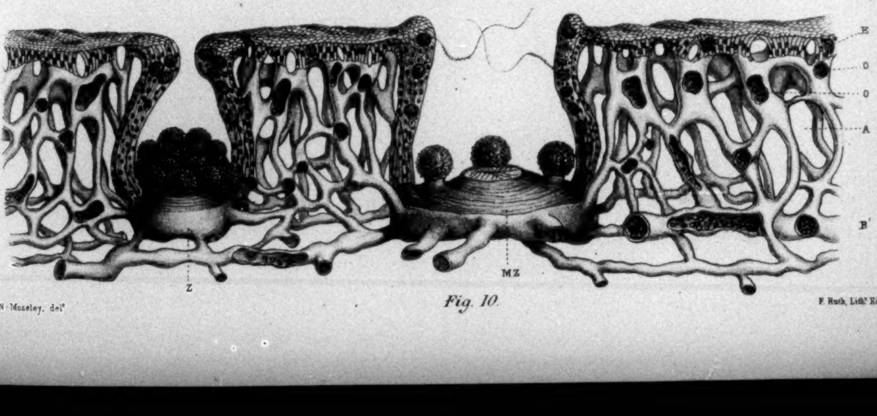

Fig. 10.

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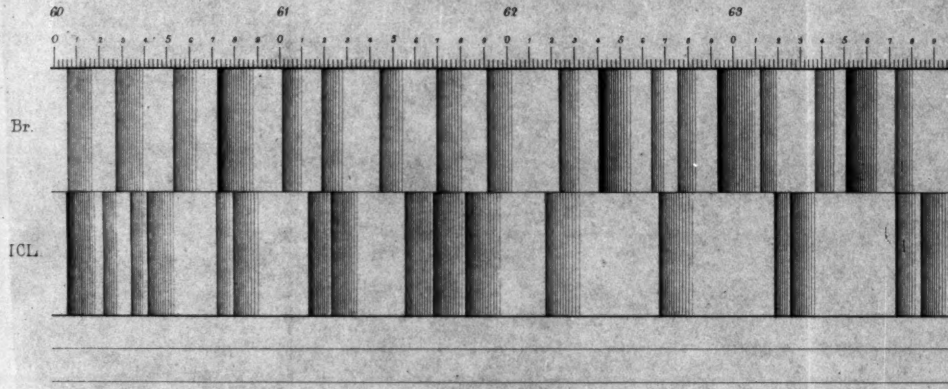
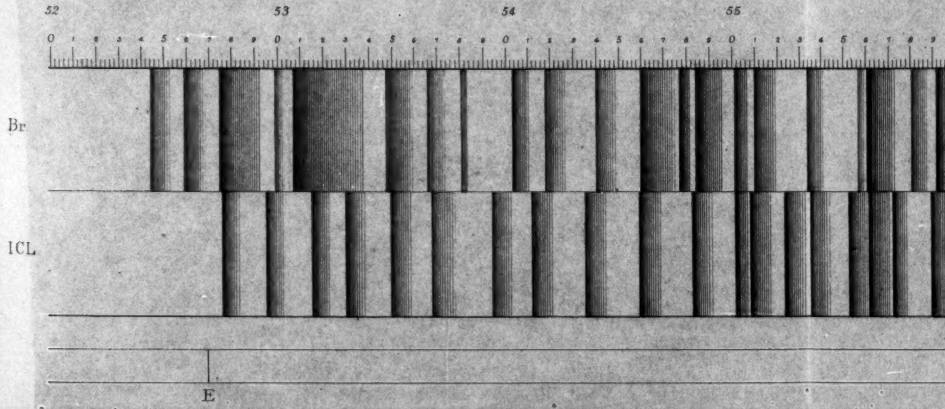
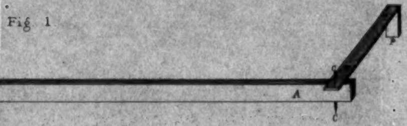
Roscoe & Thorpe.

Absorption Spectra of Bromi





mine and Iodine Mono-chloride.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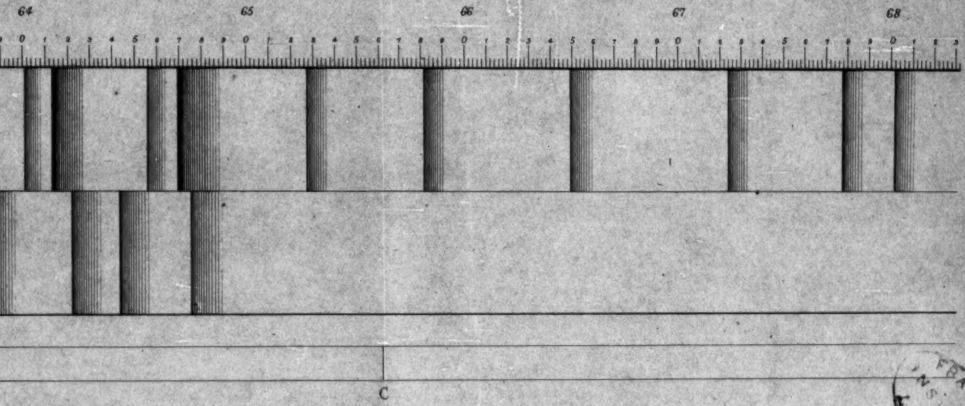

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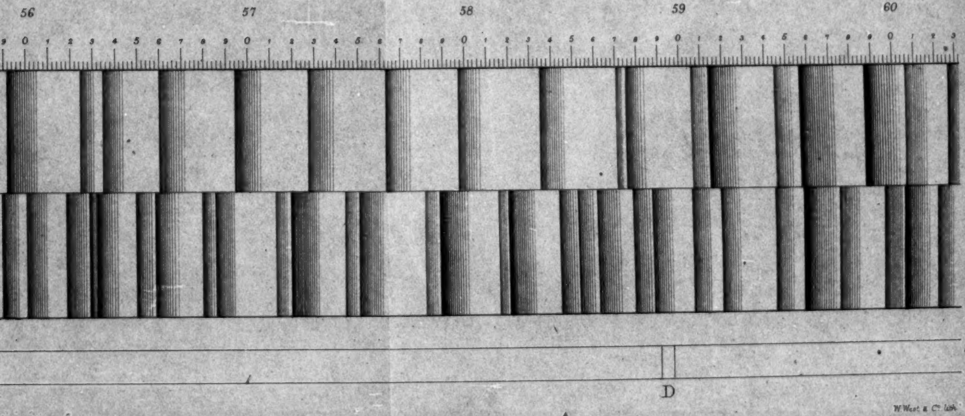

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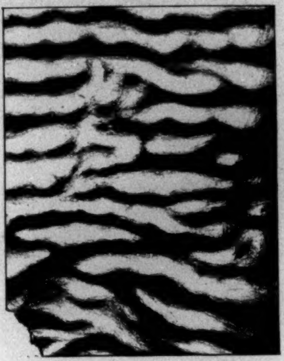

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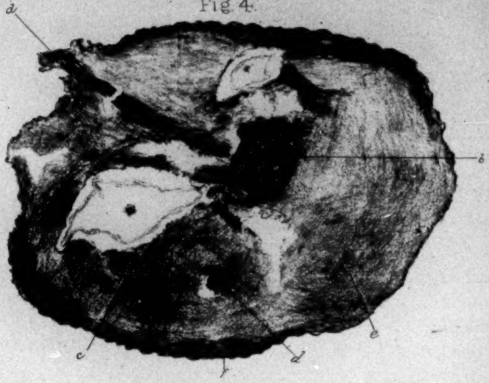

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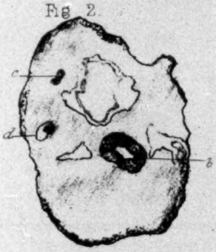

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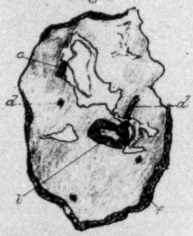

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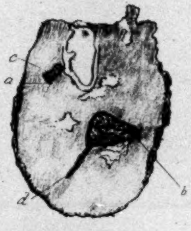
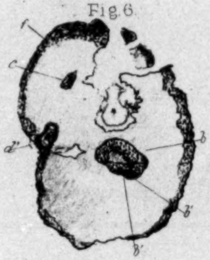


Fig. 6.



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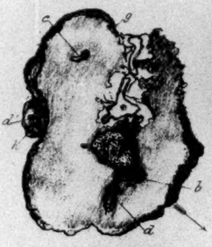

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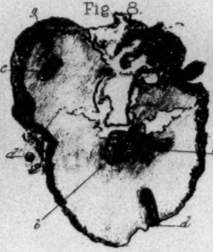

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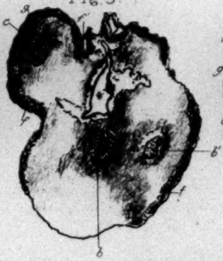

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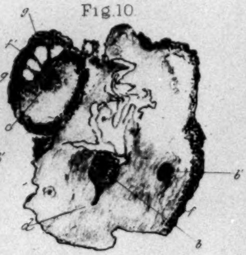

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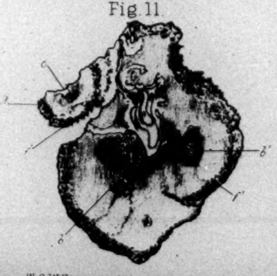

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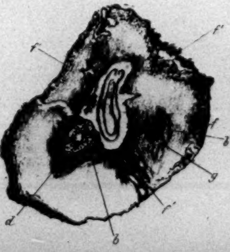

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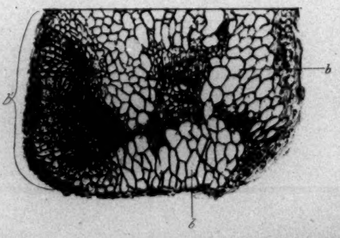

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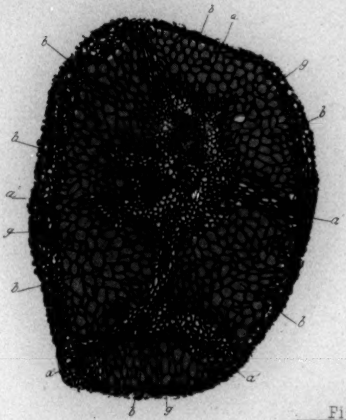
Fig. 13.



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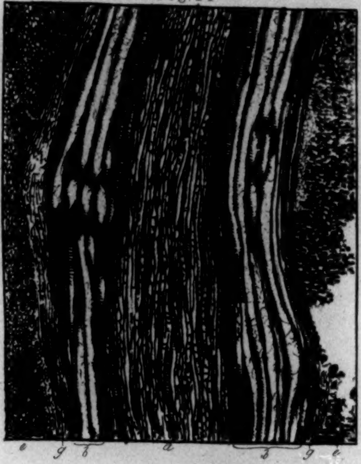

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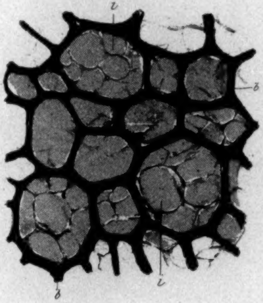

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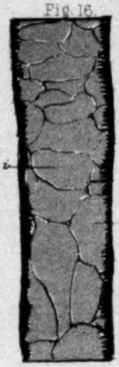

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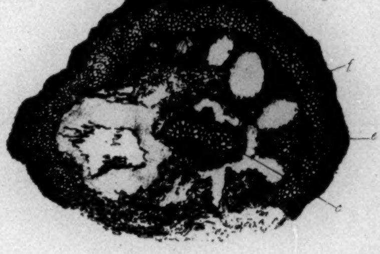

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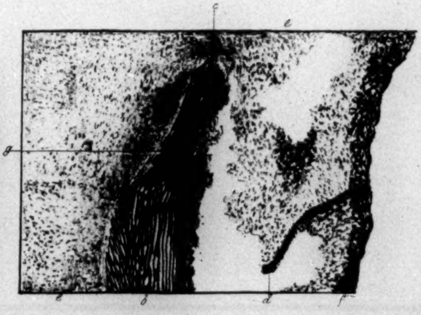

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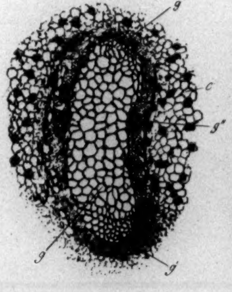

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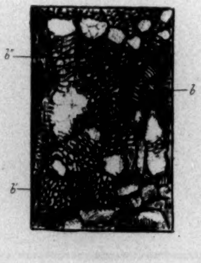

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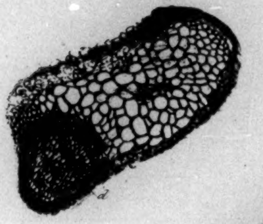

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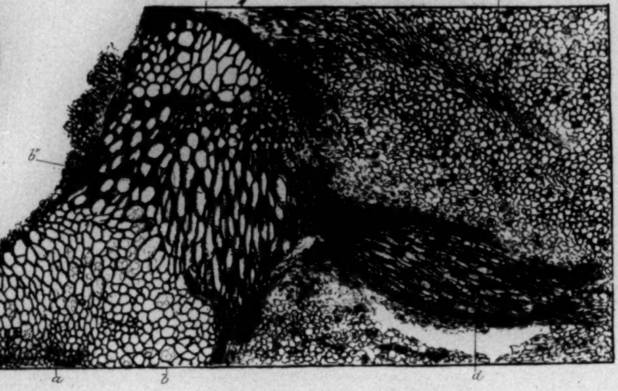

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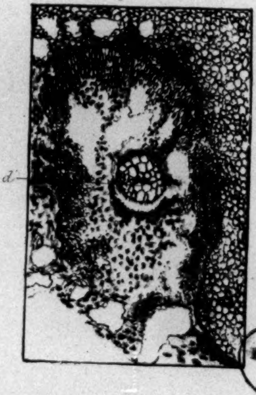

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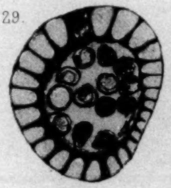

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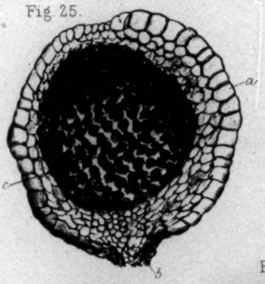

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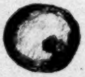

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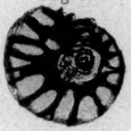

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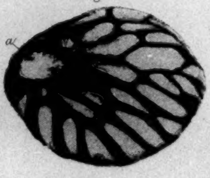

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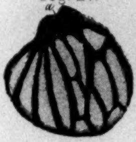

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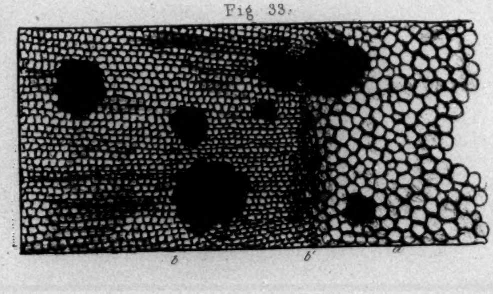

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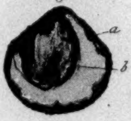

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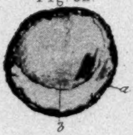

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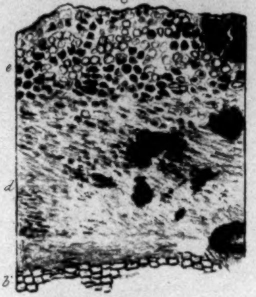

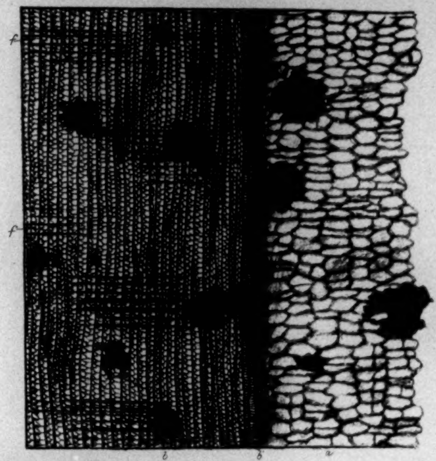

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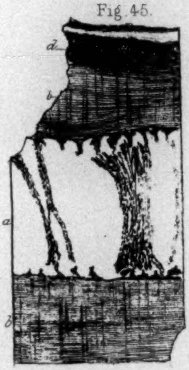

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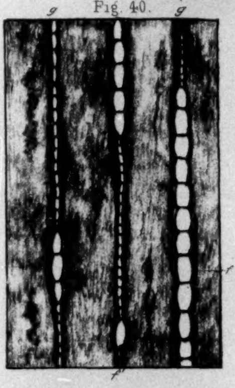

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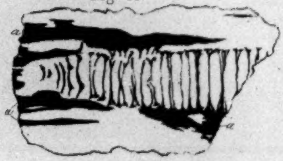

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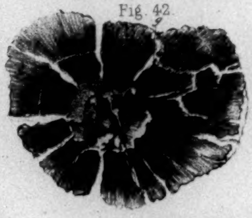

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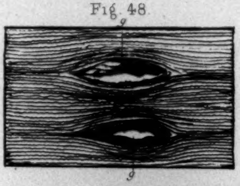

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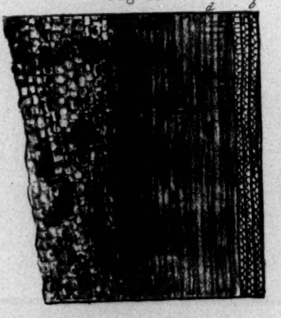

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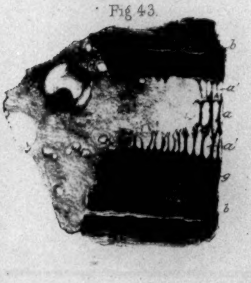

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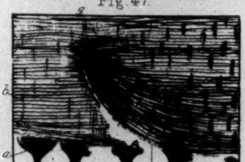

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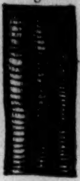

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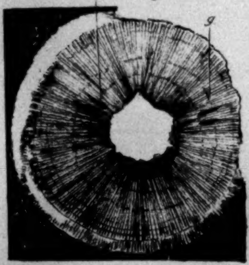

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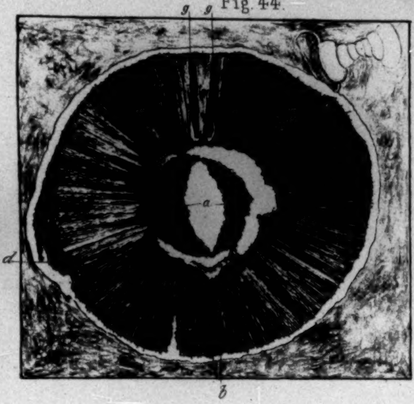

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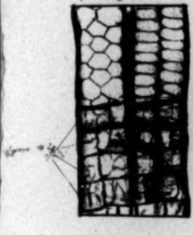

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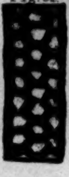

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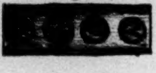

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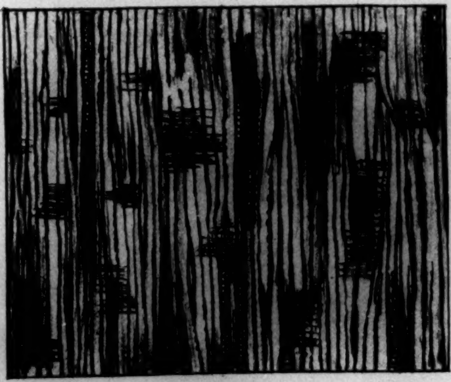

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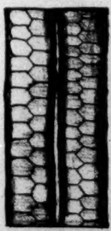

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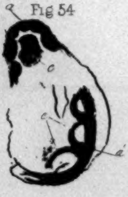

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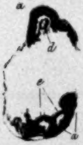

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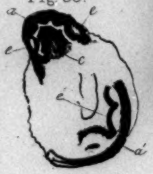

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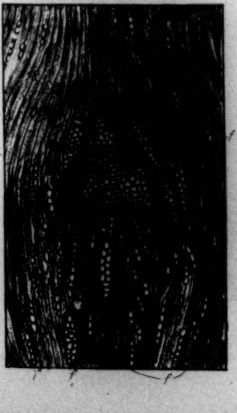

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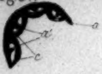

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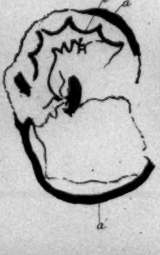

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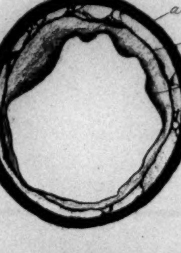

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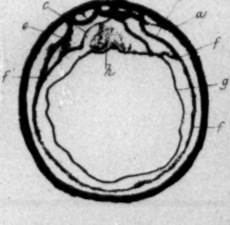

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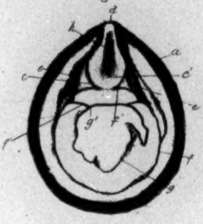
Fig. 60.



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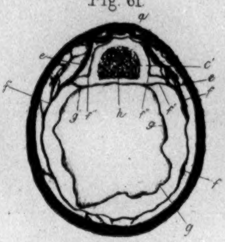

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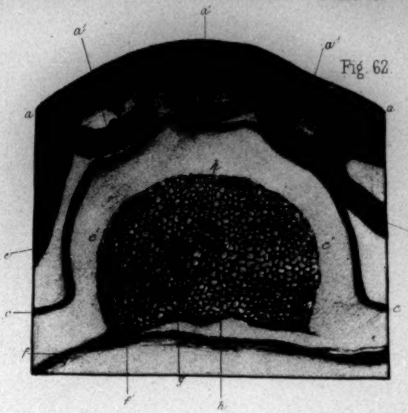

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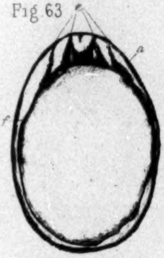

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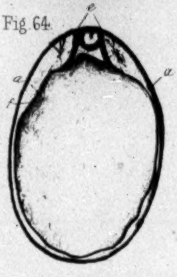

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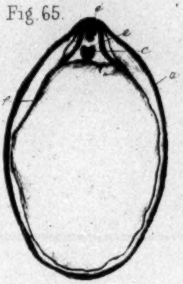

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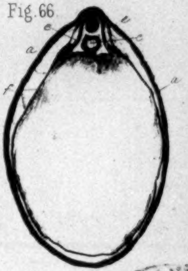

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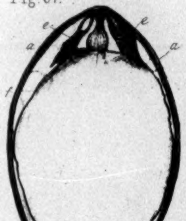

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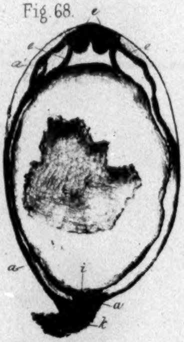


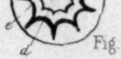
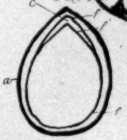


Fig. 71.

Fig 76



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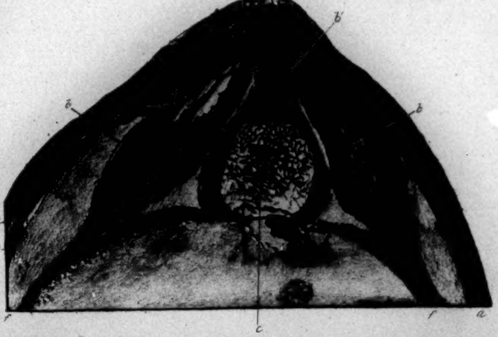

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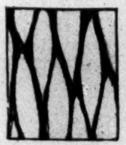

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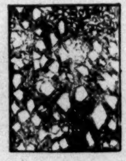

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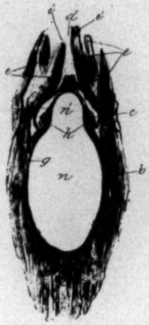
Fig 77.



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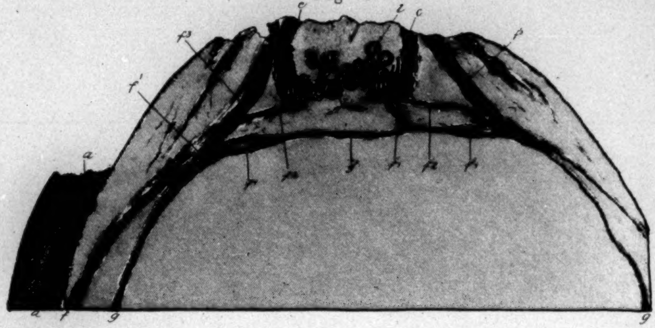

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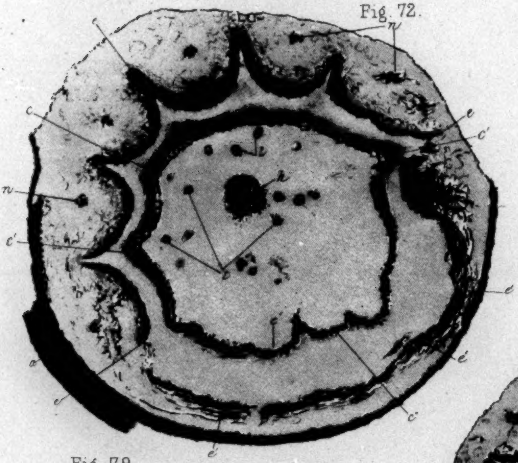
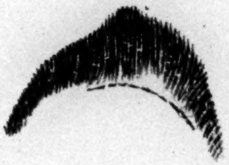


Fig. 80.*



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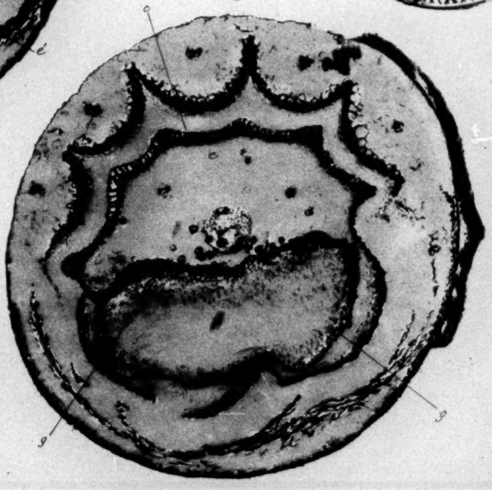

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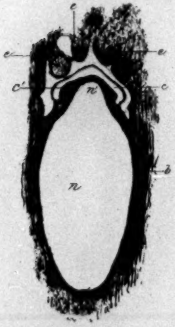
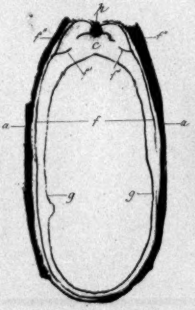


Fig 80



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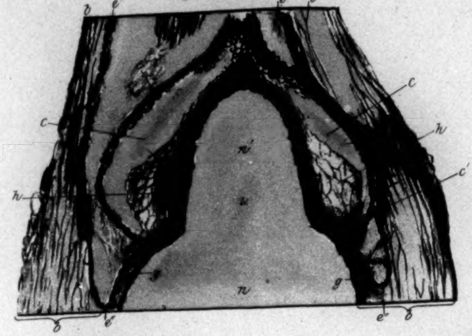

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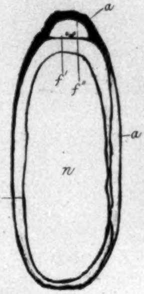

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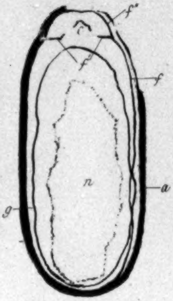

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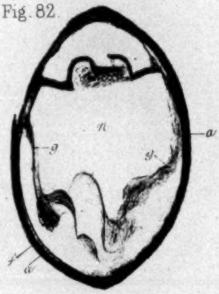
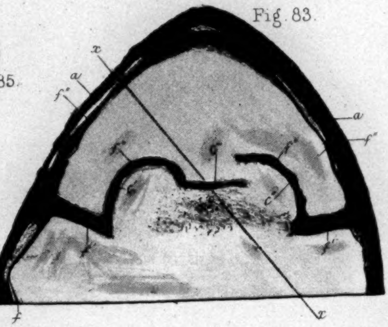


Fig. 85.

Fig. 83.



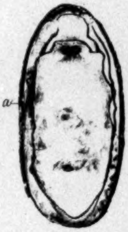


Fig. 87.

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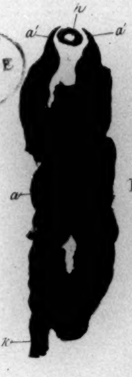

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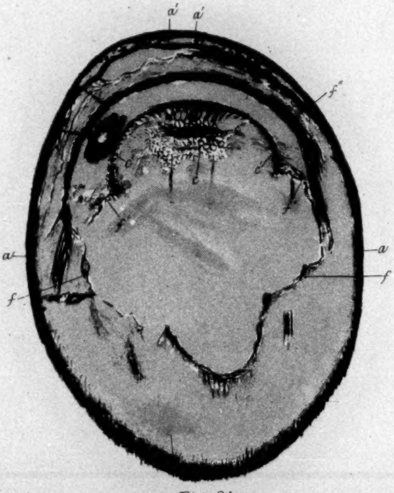

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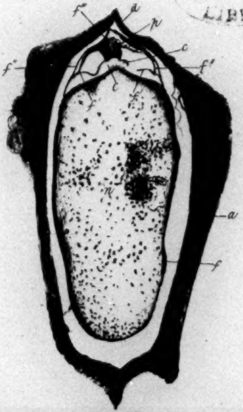
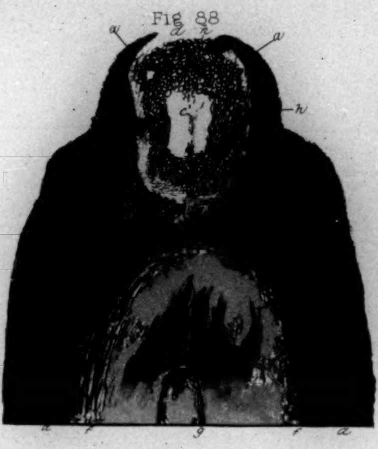


Fig. 86.

Fig 88



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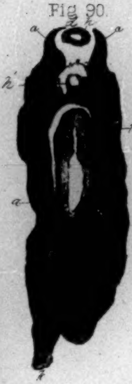

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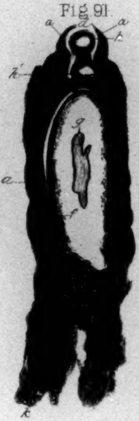

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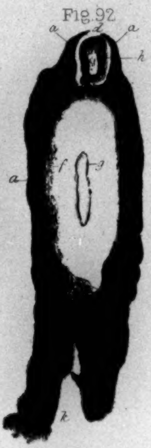

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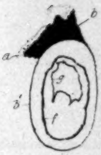

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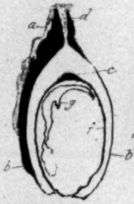

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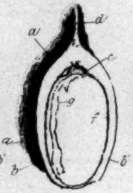

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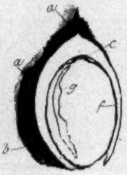

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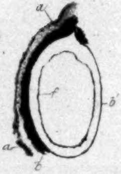

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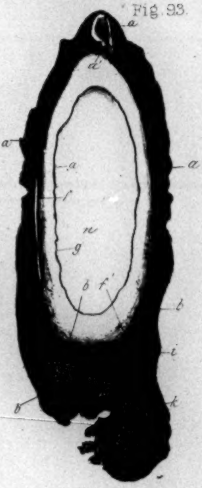
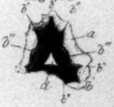


Fig 100



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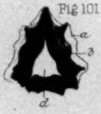

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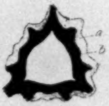

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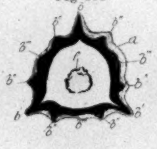

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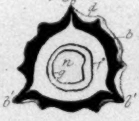

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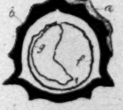

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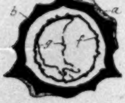

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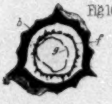

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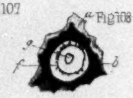

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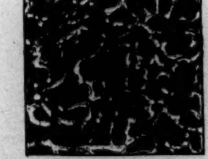

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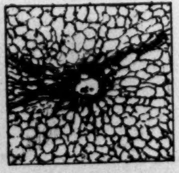

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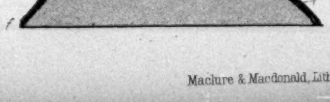

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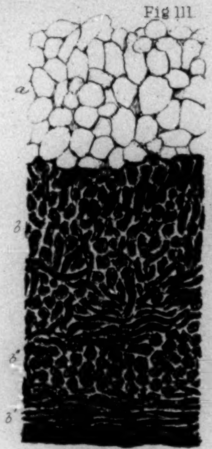
Fig 111.



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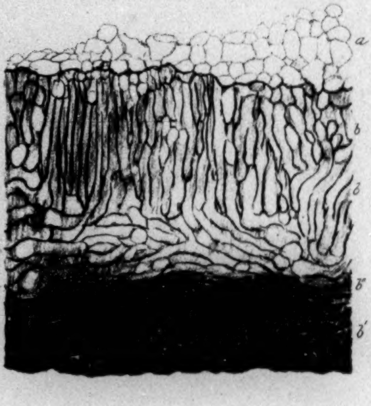

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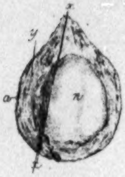

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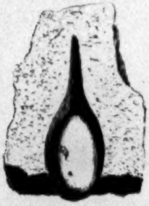

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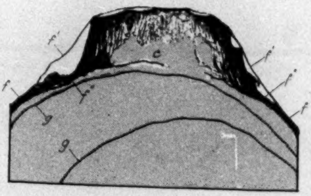

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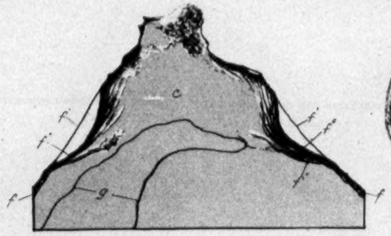

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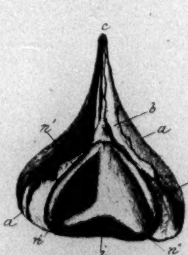
Fig 116



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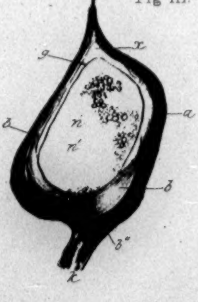

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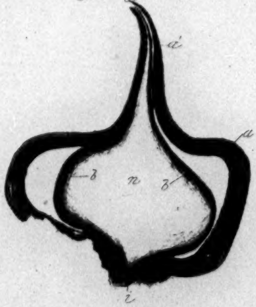

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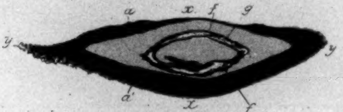
Phil. Trans. 1877. Plate 15.

Fig 124.



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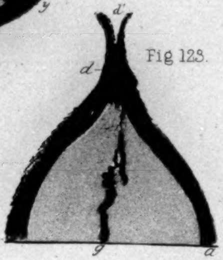

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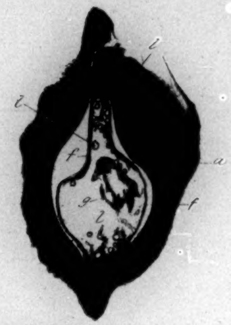

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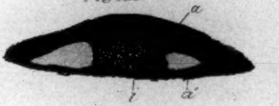

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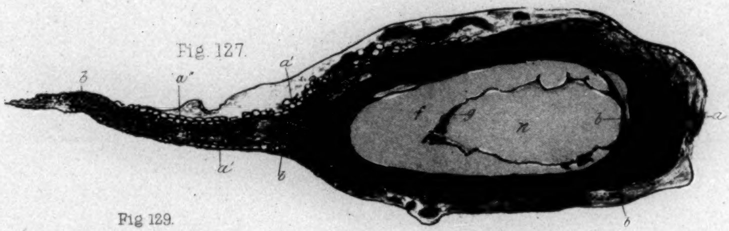

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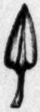
Fig.122.

Fig 129.



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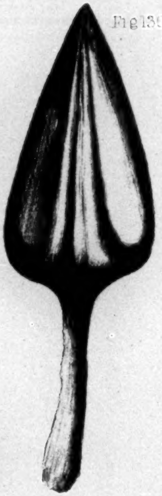

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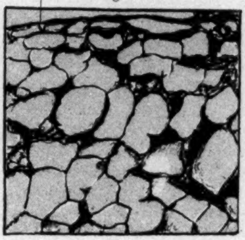

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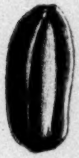

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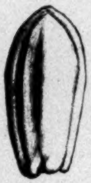

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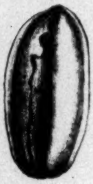

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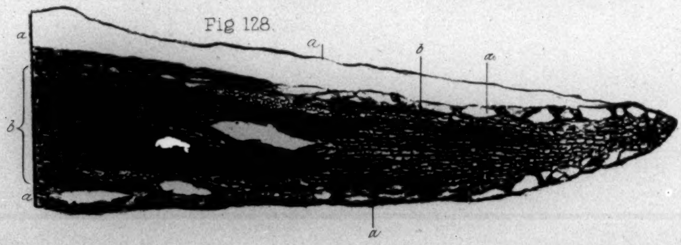

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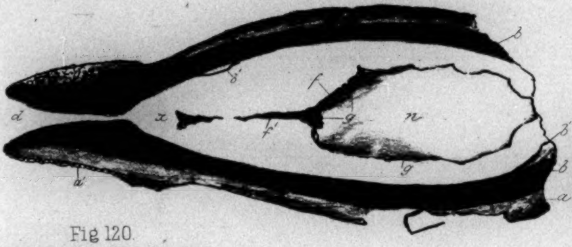
Fig 126.



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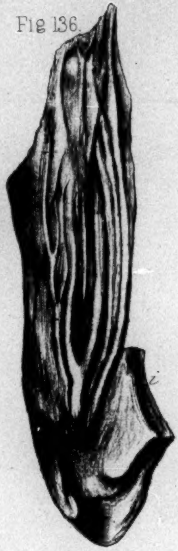

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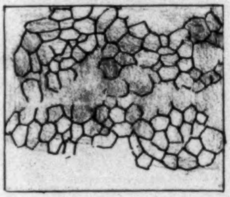

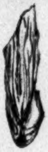

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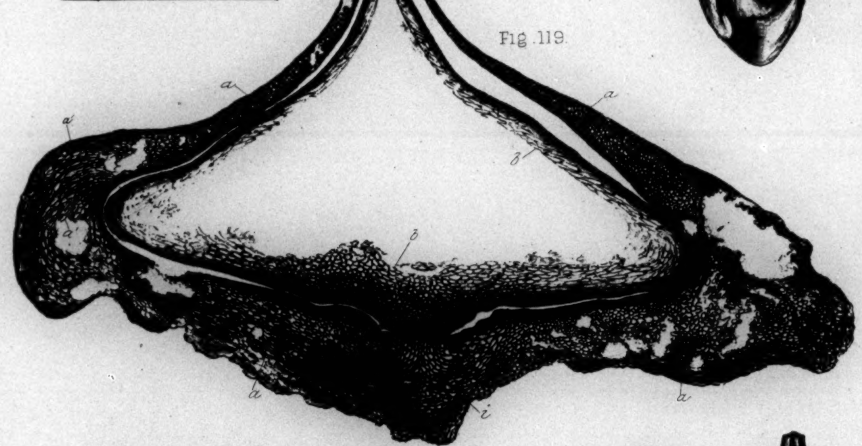
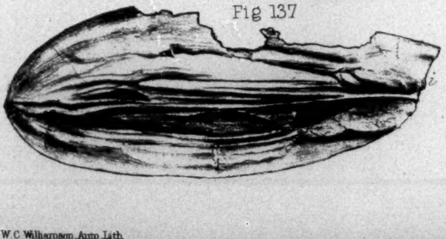


Fig .119.

Fig 137



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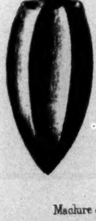

Fig115 a

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